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ANNALS OF PHILOSOPHY;

OR, MAGAZINE OF

CHEMISTRY, MINERALOGY, MECHANICS

NATURAL HISTORY,

AGRICULTURE, AND THE ARTS.

BY THOMAS THOMSON, M.D. F.R.S. L. & E. F.L.S. &c.

MEMBER OF THE GEOLOGICAL SOCIETY, OF THE WERNERIAN SOCIETY, AND OF THE IMPERI
MEDICO-CHIRURGICAL ACADEMY OF PETERSBURGH.

VOL. II.

JULY TO DECEMBER, 1813.

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PREFACE.

THE termination of the Second Volume of the *Annals of Philosophy* will enable our readers still more completely to understand the plan and the object of the work, and in what particulars it differs from other scientific journals.

It will be admitted, we conceive, without hesitation, that the second volume is considerably more valuable than the first; a sufficient proof that the attention of the Editor has not relaxed, and that more copious sources of information have been continually opening upon him.

The great increase of original communications from men of science, sufficiently conspicuous in this volume, has rendered it necessary to deviate occasionally from the original plan, as far as regards the regular insertion of the analysis of a scientific book in each Number. Such deviations cannot always be avoided; and when we have to balance between deferring an original paper and the review of a book, we conceive it but reasonable that the latter should yield to the former.

We may now confidently anticipate an unrestrained communication between Great Britain and all the other countries in Europe where science is cultivated. The Editor fully expects to derive much valuable information from the labours of the philosophers in Germany and the North of Europe: and proper means will be taken, as soon as the channels of communication are open, to secure a regular correspondence in these countries.

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ANNALS

OF

PHILOSOPHY.

JULY, 1813.

ARTICLE I.

Biographical Account of M. de Lagrange.

M. DE LAGRANGE, the greatest mathematician of the present age, having died about two months ago at Paris, the British public will no doubt be eager to get some information respecting the life and labours of so celebrated a philosopher. This has induced me to translate the following short sketch from the Moniteur of the 4th of May. It contains a general outline, which seems to be drawn with considerable accuracy and some discrimination. As soon as a more complete biography of this illustrious man is published, I shall not fail to make it known to my readers.

M. de Lagrange was born at Turin, on the 25th of January, 1736. He began his studies in that city, at a very early age; and though he made a distinguished figure, he did not at first display that strong predilection for mathematics which animated him during the greatest part of his life. The elements of that science was at that time taught in what was called the *Class of Philosophy*. M. de Lagrange attended it the first year without pleasure, and without advantage. Literary pursuits were more to his taste; but finding himself under the necessity of attending the Class a second year with more application, his mathematical genius awoke, and he made the most rapid progress. At the age of 16 he was mathematical professor to the School of Artillery. His first researches were directed to the determination of the propagation of sound. They were published in the Memoirs of the Turin Academy for 1759 and 1760. Already he had engaged in a correspondence with the great Euler, though it must be acknowledged that the letters of Euler were those of a master to his scholar; but all inequality between them speedily disappeared. At the age of twenty-four Lagrange published his *Calculus of Variations*, which had been some time in his possession. It was the greatest degree of generalization and extension that could be given to the important discovery of Descartes on the analysis of indeterminate quantities. It was received with admiration. Euler especially, who had written on the same subject one of his best works, and who had reason to regret that an idea so simple and so fruitful had escaped him, was the first person to point out and celebrate the method of his young rival. He wrote several papers by way of commenting on it; and it was he that gave it the name of the *Calculus of Variations*. Lagrange himself was satisfied with giving it the name of an *essay*.

Lagrange did justice to this great honour, by acquiring new titles to glory. In 1764 he gained the prize proposed by the Academy of Sciences on the *libration of the moon*. Not only did his analysis embrace the whole of the question proposed, but he likewise pointed out to mathematicians the extent and fecundity of the principle of virtual velocities in the solution of mechanical problems. This idea contains the germ of one of his finest works, which he called *Mechanique Analytique*, because he reduced under a single analytical formula all imaginable mechanical questions, supposing the direction and the mode of action of the forces known. M. de Lagrange gained four other great prizes proposed by the Academy of Sciences. We would not properly appreciate the importance of this circumstance unless we attend to the nature of the questions proposed in such cases. They consisted of the most important points of science, of the most difficult and profound theories towards which the efforts of mathematicians were drawn. We may almost reckon the steps made by science by the number of such questions proposed and resolved.

M. de Lagrange quitted his country in 1766. Euler, who had been director of the Berlin Academy, went at that time to Petersburgh. Frederick the Great proposed to d'Alembert to succeed him. D'Alembert returned his thanks to the King, and pointed out Lagrange as a proper person to fill the place. Lagrange, accordingly, was chosen. His arrival in Berlin was marked by a fine work on numerical equations, which constitutes the foundation of the treatise that he afterwards published on that subject. Soon afterwards he communicated his researches on algebraic equations; and during the 22 years that he continued Director of the Berlin Academy he published about 60 dissertations on all parts of mathematics, on partial differences, finite differences, probabilities, the theory of numbers, and the most difficult questions of general astronomy. This did not prevent him from sending likewise papers to the Turin Academy, which had been honoured by his first successful efforts. No man possessed of less extent of genius, and less fecundity of invention, could have ventured to succeed such a philosopher as Euler; but every person must acknowledge that Euler had a worthy successor.

During his residence in Berlin, M. de Lagrange married, not so much from any inclination for the state, as because it was customary for the academicians to be married. This union was followed by several misfortunes. M. de Lagrange had a child, who died, we are informed, while young. His wife died, likewise, after a tedious and painful disease. M. de Lagrange took care of her during her illness with the most inviolable attachment, hardly ever leaving her, and contriving new methods for her cure. This second loss rendered his abode in Berlin disagreeable: besides, he was afraid that the tranquillity of Prussia would be interrupted. These motives caused him to listen to the offers which were made to induce him to go to France, where he hoped to enjoy greater tranquillity. He reached that country in 1787, and was soon after surprised by the Revolution. He passed through it without experiencing any personal misfortune. About this period his *Mecanique Analytique* appeared. M. de Lagrange had sent the manuscript from Berlin, and had entrusted the publication of it to one of the most celebrated French mathematicians. It had been printed for two years before Lagrange even thought of opening it; and when a gentleman to whom he communicated the circumstance expressed his astonishment at so much indifference, "I was disgusted," said he, "with these kinds of combinations, and I set myself to learn chemistry, which I now find easy, for it may be learned in the same manner as algebra." It is necessary to be a Lagrange to seek in algebra a model of facility. It is remarkable that the taste for mathematics may be thus destroyed, and revive again. D'Alembert seems to have undergone the same kind of change.

In 1792 M. de Lagrange married, a second time, a young and beautiful lady, daughter of M. Lemonnier, one of his fellow members of the Academy. She rendered his life very happy. He observed in his last moments that he found death easy, and that his regret in leaving an excellent wife could alone make it painful. When, after the events of Thermidor, public instruction was again re-established, M. de Lagrange was named Professor in the Normal School. The lectures which he there delivered have been printed. When the Polytechnic School was formed, he was likewise one of its first Professors; and those who had the happiness to hear him know with what respect he was listened to. It was then that he published his *Calcul des Fonctions Analytiques*, his *Traité des Fonctions*, and his *Resolution des Equations Numériques*. These works, composed for the Polytechnic School, were not one of the least causes of its celebrity. When the Institute was formed M. de Lagrange was necessarily named the first member in the section of Geometry. When the Board of Longitude was established he was appointed one of its members; and till the very last period of his life, nobody was more exact than he in his attendance at the meetings of both these learned bodies.

At the epoch of the 18th Brumaire he was named Senator, and successively Grand Officer of the Legion of Honour, and Grand Cross of the Order of Reunion. The eclat of rank and fortune did not seduce him for a moment. He retained always the same mode of life, the same habit of study, the same simplicity. This wise conduct was the more necessary for him, because he had always been of a feeble constitution; and it was to this extreme moderation, in every thing but study, that we must ascribe the length of his life, and his old age free from infirmity. He had likewise the rare good fortune to preserve his genius to the end of his life. Indeed, if we examine the whole of his works, we shall find in them marks of the progress of the science, but no indication of old age. He had undertaken at the latter period of his life to give a new edition of the *Mecanique Analytique* considerably augmented. He published the first volume, in which, among other remarkable additions, we admire his fine investigations of the most general questions of astronomy and mechanics. He laboured with the most indefatigable industry at the two remaining volumes, in which he intended, it is said, to treat of the great phenomena of the system of the world; but this labour hastened the period of his death. It is said that the manuscript of the second volume exists, written entirely with his own hand. It is to be wished, for the good of the sciences, that the publication of this precious monument be committed to persons who will acquit themselves with promptitude and fidelity.

The character of the genius of Lagrange has been exactly appreciated by a philosopher whose name in the sciences has been long associated with his own. If we durst add any thing to that judgment, it would be to confirm it, by recalling to memory the impression made upon the mind by the perusal of the works of Lagrange. It is not only the pleasure that results from a clear and accurate arrangement, it is a ray of light which darts upon the mind, removes the obscurity from the most complicated objects, and discovers to your astonished eyes the certain and direct road which leads to the object that you wish to obtain. When we have once read and understood a memoir of Lagrange, we have never any occasion to recur to it again; we have learned the whole, and never can forget it. In this generality of his views he rises above Euler. Euler, indeed, possesses other advantages: in the immense variety of his works he lays open a multitude of extraordinary means, and a fertility of invention, which nothing can stop. Mathematicians, by reading him, learn all the secrets of the science of mathematics; but M. de Lagrange alone can offer them the model of that perfection, almost ideal, which we ought to endeavour to attain.

Notwithstanding what we have said, we should leave a very imperfect character of Lagrange, if we did not notice his wit. He possessed it in such perfection that it alone would have raised the reputation of any other person but M. de Lagrange. What a turn of thinking must he have had, who, by way of relaxation from the most abstract studies, made choice of the history of religion and of medicine! It is true that in consequence of this investigation he lost all confidence in medicine; but this scepticism was so simple and tolerant, that if it was an error it was impossible not to forgive it. This philosopher, who knew so many things, was exceedingly ready to acknowledge his ignorance. These simple words, *I do not know*, were his favourite expression. He generally began and finished in this manner the statement of his doubts. He was not apt to be satisfied with words, nor to stop at the surface of things. He deprived opinions and things of the envelope with which they are usually covered; and when he had thus exposed them naked, he gave his thoughts respecting them, usually in an original and lively manner, as remarkable for depth of sense as for fineness of expression. Many of his sayings are well known. One of his friends was speaking to him of an opinion which, alternately adopted and rejected, admitted and modified, by philosophers, had become at last a popular prejudice. "What!" said M. de Lagrange, "are you astonished at that? It is the very thing which always happens. Prejudices are nothing else than the cast clothes of philosophers, in which the rabble dress themselves." We state this anecdote because it points out well the nature of his observations.

Though his figure was good, he would never permit his portrait to be drawn. He thought that the productions of the mind were alone entitled to survive. If his face remains unknown, the remembrance of his genius will last as long as civilization continues to dwell upon earth.

Article II.

Further Observations and Experiments on the combinations of Oxymuriatic Acid with Lime. By John Dalton.

In my essay on the oxymuriate of lime (inserted in Vol. I, No. I. of the *Annals*), I gave my opinion that the *muriate of lime* found in the oxymuriate of commerce was accidental, and not essential to the salt. I have lately been favoured with a specimen of oxymuriate of lime, immediately after its formation, by Dr. Henry. Oxymuriatic acid gas was passed through a vessel containing a portion of water into another vessel containing hydrate of lime, in the proportion of three parts lime to one of water nearly; this was continued till the hydrate was saturated. On the oxymuriate thus prepared I made a few experiments, which afforded me additional and satisfactory information on the subject; particularly as they confirm the opinion that muriate of lime is not essential to the constitution of the oxymuriate.

Exper. 1.—To 100 grains of the salt were added 1000 grains of water; after due agitation and filtration, a liquid was obtained of the sp. gr. 1·041; a residuum of 21 grains, dried in 100, was also obtained; this, treated with test nitric acid, indicates 16 grains of lime. One-fifth of the former liquid (1·041) was treated with test carbonate of soda, and boiled; it threw down 8 grains of carbonate of lime, equal to 4·5 of lime. Hence the whole solution contained 22 grains of lime, of which 2 grains must have existed in the solution, as in lime-water, and have been borrowed of the precipitated lime. It appears, then, that 20 grains of lime were held in solution by acid, and 18 precipitated, of which 2 were again taken up by the water of the solution. The total lime, then, in 100 grains of the dry oxymuriate was 38 grains. This was confirmed by treating 20 grains of the dry salt with sulphuric acid, by which $18\frac{1}{2}$ grains of sulphate of lime were obtained, which correspond to $38\frac{1}{4}$ grains per cent. of lime.

Thus, then, it appears, that the above specimen gives nearly the same total of lime as that analysed in my former paper; but it contains more soluble matter, as is shewn by the greater sp. gr. of the solution.

Exper. 2.—It was found that 100 measures of the solution, 1·041, required 56 of test green sulphate of iron (1·149) to annihilate the oxymuriatic acid. The calculation from this being made as in my former, gives 23 grains of oxymuriatic acid in 100 of the dry salt. This acid would be in combination with 19 grains of lime; but from the preceding experiment it seems there were 20 grains of lime in solution. This small difference arises, perhaps, from the errors of the proportions; or, perhaps, from the liquid containing a very small portion of muriate of lime. This last idea was tried by saturating one-fifth of the liquid (1·041) by test muriatic acid, and expelling the oxymuriatic acid, as related in my former paper; but it was found that the lime required as much acid as was sufficient to saturate it, or so nearly that quantity that the difference was scarcely appreciable.

Since the former essay was written, I have found another method of determining the quantity of oxymuriatic acid in any specimen of oxymuriate; it is to dissolve a known weight of the oxymuriate in a small portion of water; then put the liquid into a graduated tube over mercury, and discharge the gas by an acid; it may thus be measured, and the quantity retained by the liquid may be estimated at twice the bulk of the liquid nearly. [*See New System of Chemical Philosophy, p.* 298.] Three grains of the dry oxymuriate above give, when dissolved in 30 of water, and 20 grains of dilute acid are added, 170 measures of gas over 50 of watery solution, which last may be presumed to retain 90 more. Hence the total gas may be estimated at 260 measures, which, of the sp. gr. 2·34, would give ·72 grain for 3, equal to 24 per cent. of acid in the salt. The gas, I find, may be kept in the tube under these circumstances for a week, without losing more than 40 or 50 measures of its volume; so that its combination with mercury is slow, if not agitated. Upon the whole, however, I prefer the green sulphate test for precision.

As nothing was in the salt but oxymuriatic acid, lime, and water, we have its constitution as under:—

23	oxym. acid
38	lime
39	water
100	

But as the hydrate of lime at first had a minimum of water, which is known to be ⅓ of its weight, it could only be 13 grains: whence, then, were derived the other 26 grains of water? This is an important question; especially as the specimen in my former paper was found to contain an equal, or greater, quantity of water (42) per cent.

Exper. 3.—Suspecting that more water was found in the oxymuriate of lime than was essential to its existence, I attempted to distil the surplus water off by a gentle heat. A small portion of the salt was put into one end of a glass tube, and a gentle heat applied; water soon dropped from the cool end; it smelled strongly of the acid. The dry salt was then examined; it proved to be lime and muriate of lime, with scarcely any trace of oxymuriatic acid.

From this experiment it would seem probable that the excess of water is necessary to the constitution of the salt. If so, the manufacturers of the article will find it most conducive to their object not to use dry hydrate of lime with a minimum of water, but lime that contains its own weight of water, or what I should call a *trihydrate*, that is, 1 atom of lime combined with 3 of water.

Referring now to the atomic system, in order to have a clear view of these compounds of oxymuriatic acid and lime, it appears that the dry oxymuriate of lime, or, as it should be called, *hyperoxymuriate*, consists of 1 atom of acid, 2 of lime, and 6 of water; namely;—

1 of oxym. acid	29	or	23·2
2 of lime	48		38·4
6 of water	48		38·4
	125		100

When the salt is dissolved in water, one-half of the lime is precipitated, and the liquid contains a solution of *oxymuriate* of lime; the proportions of the elements of acid and base being, then,

1 atom of acid	29	or	54·7
1 atom of lime	24		45·3
	53		100

When the liquid solution has a current of oxymuriatic acid gas sent through it till it becomes saturated, then the liquid contains a solution of *hyperoxymuriate* of lime; the proportions of the elements being, then,

2 atoms of acid	58	or	70·7
1 atom of lime	24		29·3
	82		100

N.B. The last term is not used here in the same sense as is now ordinarily received; it will probably be found expedient hereafter to designate such compounds as are now called *hyperoxymuriates* by a different name.

Article III.

Description of a Resinous Substance lately dug out of the Earth at Highgate. By Thomas Thomson, M.D. F.R.S.

During the late attempt to make a tunnel through Highgate-hill, in the neighbourhood of London, a very considerable number of curious fossils were discovered. The beds dug through consisted partly of gravel and partly of clay. The number of shells thrown out, and the round masses of limestone, could not escape the most careless observer; but one of the most remarkable substances detected was a resinous body, in shapeless masses of various sizes. Mr. Sowerby had the goodness to send me some specimens of this curious substance; and as I am not aware that any account of it has hitherto been published, I conceive that the following observations which I made upon the specimen that I received will prove acceptable to my chemical and mineralogical readers.

I. The colour of Highgate resin is of a dirty yellowish light brown. It is semitransparent. Its lustre is resinous, and its surface smooth; though not perfectly so; but having the appearance of having been rubbed, as would have happened had it been mixed with gravel upon the margin of the sea-shore, or a lake.—Brittle; not so easily broken as common resin; but much more so than copal: softer than copal; has a resinous and aromatic smell, especially when heated; this smell is peculiar, though it has some faint resemblance to the smell of camphor.

II. Its specific gravity at the temperature of 60° is 1·046. This agrees almost exactly with the specific gravity of copal as determined by Brisson; but on trying the specific gravity of copal, I found it 1·069. Hence either copal differs considerably in its specific gravity, or the resin called copal by Brisson was not the same to which we give that name in Britain.

III. When heated it melts, and may be rendered as liquid as water without alteration in its colour. It catches fire at the flame of a candle, and burns with a clear yellow flame, and emitting abundance of smoke, as is the case with other resins. At the same time it emits a strong aromatic odour.

IV. When in lumps it is insoluble in all the reagents I tried, namely, water, alcohol, potash ley, acetic acid; except ether, nitric acid, and sulphuric acid, which act upon it more or less.

Ether renders it opake, and white, and quite tender; so that it has lost its cohesion, and crumbles into powder upon the least pressure between the fingers. The ether at the same time dissolves a portion of it which it deposites, and becomes milky when agitated with water.

Nitric acid acts upon it slowly when assisted by heat, and partly dissolves it, and partly converts it into a red coloured substance. The acid itself becomes red, and when diluted with water lets the resin again fall in white flocks. These flocks when dry are in the state of a light yellow coloured powder, having a bitter taste. I could not dissolve it in water; but it dissolved in alcohol, at least as easily as the unaltered resin.

Sulphuric acid readily chars this resinous body when assisted by heat.

V. When reduced to the state of a fine powder alcohol readily dissolves a small portion of it, and lets it fall again when mixed with water; but alcohol is a bad solvent of this resinous body. The same observations apply to ether.

VI. I cannot find that either potash or subcarbonate of potash dissolve this resin, though boiled with it for some time in the state of powder. This is the property which distinguishes Highgate resin from every other with which I am acquainted. Even amber is partially acted upon by alkaline leys, and tinges them yellow very speedily.

VII. Nor do I find that acetic acid dissolves any perceptible portion of this resin after a week's digestion in it, when in the state of a fine powder. I even triturated them together for a considerable time in a mortar, and then boiled them in a glass tube, but no solution was effected. Here, again, another character which Mr. Hatchett has assigned to the resinous bodies fails when applied to the Highgate resin.

VIII. I have not tried the action of oils upon Highgate resin; but from the properties above described I conceive there is reason to presume that, like copal, it will not dissolve in any of them.

IX. It burns all away before the blow-pipe upon a piece of metal without leaving any perceptible ash behind it, when we make choice of pieces quite free from any earthy matter attached to them.

Such are the properties of this substance, as far as I have examined them. They are sufficient, I think, to distinguish it from all the vegetable substances hitherto observed. It approaches nearest to copal and amber; but is distinguished from the first by its solution in alcohol, and its non-solution in potash ley; from the second, by its readily melting when heated, and by its melting without any perceptible change of its properties. Thus the chemical properties of this singular substance throw no light upon the source from which it was derived; and cannot, therefore, facilitate our inquiries into the revolutions which the southern part of this kingdom has undergone, and the various animal and vegetable remains so thickly scattered in its bowels.

Article IV.

On a new Variety of Ulmin. By Thomas Thomson, M.D. F.R.S.

The experiments detailed in the first number of these *Annals* upon the ulmin from the elm show us that it is a peculiar vegetable principle, distinguished by the following properties:—

1. Of a dark brown colour, and little taste.
2. Soluble in water, but insoluble in alcohol and ether.
3. Not precipitated by solution of gelatine.
4. Precipitated brown by iron, tin, mercury, and lead, when these metals are in the state of saline solutions.
5. Precipitated by acids.
6. Swells greatly when heated, as is the case with gum, but does not melt.

These characters do not apply to any other vegetable principle at present known. Hence it is obvious that we must constitute this substance a new vegetable principle. Some chemists are inclined to refer ulmin to *extractive*; but it is high time to render that very indefinite class of vegetable substances somewhat more precise. Nobody has ever examined extractive in a state of purity. Hence its properties are unknown, and it has been customary to refer to that class all substances which could not be referred to any other. It constitutes a kind of sink, or common sewer, in vegetable chemistry; but such an indefinite mode of proceeding is highly injurious to the progress of this branch of the science. If we wish to make ourselves accurately acquainted with the vegetable kingdom, we must distinguish every substance which possesses peculiar properties by a peculiar name. No risk of error results from multiplying the number of vegetable principles: the error into which we are most likely to run is classing the most dissimilar substances under the same name; thus enabling us to satisfy ourselves and others with giving a substance a name without being in the least aware of its distinguishing characters. What, for example, could be more preposterous than to give the same name to vegetable substances, some of which are soluble, and some insoluble, in alcohol? Yet this has been proposed by some of the most eminent chemists, both in this country and on the continent.

Now that the vegetable principle which constitutes the subject of this article has been distinguished by a name, and that the attention of men of science and observation has been turned to it, we may expect to find different species of it exuding from different species of trees besides the elm. The variety which I am going to describe is an example of this. I got it from Mr. Sowerby, who informed me that he collected it from the *oak*, It possesses the following properties, which distinguish it from the ulmin of the elm:—

1. Its colour is a very dark brown, almost black, and it leaves a chocolate coloured stain on paper. It readily crumbles to powder between the fingers. Lustre, resinous. Taste, more astringent than the ulmin from the elm, and inclining to bitter.

2. Dissolves readily in water. The colour of the solution is dark brown, and so intense as to be opake. When left to spontaneous evaporation upon a watch-glass the ulmin remains, divided into a great number of minute portions by sections, which issue in rays from the centre, and the ulmin adheres but weakly to the glass.

3. Insoluble in alcohol and ether.

4. When the aqueous solution is dropped into alcohol of the specific gravity 0·809, brown flocks precipitate, but the alcohol retains a brown colour, and of course a portion of the ulmin remains in solution.

5. The aqueous solution is not precipitated by the solution of gelatine in water.

6. Some of the precipitates which the aqueous solution of this variety of ulmin forms with the metalline salts differ in colour from the precipitates formed by the ulmin from the elm. The following were the metalline salts tried:—

(1.) Sulphate of iron is thrown down dark green, and the colour is permanent, though the liquid be left 24 hours in an open glass.

(2.) Sulphate of copper is precipitated also green.

(3.) Sulphate of zinc is precipitated brown, and the colour of the precipitate speedily deepens, and becomes at last a dirty black.

(4.) Nitrate of silver is precipitated in brown flocks.

(5.) Acetate of lead is precipitated in brown flocks.

7. The aqueous solution is precipitated in brown flocks by a few drops of nitric acid. This solution being evaporated to dryness in a watch-glass, a yellow tasteless powder remained, insoluble in alcohol, but soluble in water. This powder was charred at a very moderate heat; owing probably to the action of the nitric acid which it still retained.

Acetic acid does not precipitate this variety of ulmin from water.

8. Neither potash, carbonate of potash, nor ammonia, precipitate it from water.

9. When exposed to heat it swells up like gum, and readily burns away before the blow-pipe, leaving behind it a minute portion of white matter, which did not melt by the continuation of the heat. This matter dissolved with effervescence in nitric acid, except some hardly perceptible flocks, which had the

aspect of silica, but were too minute to be subjected to a chemical examination. The addition of ammonia to the nitric acid solution occasions the separation of a few flocks, which redissolve by agitation. Carbonate of potash occasions a more copious precipitate. Hence it would appear that the ash consists of carbonate of lime, with some traces of magnesia and silica.

This variety of ulmin resembles the bitter principle from coffee described by Chenevix, and the tannin of Kino, in striking a green colour with sulphate of iron. Its effect upon sulphate of zinc I consider as its most remarkable property. Zinc is usually precipitated of a white colour from its solutions; but the ulmin of the oak throws it down almost black.

The appearance of the ulmin of the oak, its taste, and the tree from which it was obtained, led me to expect that it would contain tannin; but if not forming a precipitate with gelatine be characteristic of the absence of that principle, as we consider it at present, we must conclude that the ulmin of the oak contains no tannin whatever.

Mr. Sowerby likewise collected ulmin from the hornbeam; but as he unfortunately mixed it with ulmin from the elm, it was not possible to determine its peculiar characters. I mention the circumstance to induce such of my readers as are interested in the progress of vegetable chemistry, and have an opportunity of examining the trunks of trees, to look for exudations from them, that we may have information as speedily as possible of the various trees that yield this hitherto neglected vegetable principle.

ARTICLE V.

On Sir H. Davy's Theory of Chlorine, and its Compounds.[*] By Mr. William Henderson, Member of the Royal Medical Society of Edinburgh.

THE reasons which may be alleged in proof of the simple nature of oxymuriatic gas seem easily reducible to four heads, viz. :—

I. It is converted into muriatic acid by union with hydrogen; and this change is unaccompanied by the evolution of any aqueous vapour.

II. The products of its action on combustibles, and on metals, differ essentially from those which arise from the action of oxygen on the same bodies.

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[*] This essay was honoured with the prize medal of the Medical Society of Edinburgh for 1812.

III. It cannot, when perfectly dry, be made to act on, or unite with, charcoal.

IV. In most, if not in all cases of its evolution, a portion of water is formed.

Each of these will be the subject of separate examination.

I. *Of the conversion of Oxymuriatic Gas into Muriatic Acid, by the action of Hydrogen.*

It is stated in Dr. Thomson's System of Chemistry,[*] that water is an essential ingredient of muriatic acid; and that, hitherto, all attempts to procure it in an insulated elastic form have failed. M. Berthollet remarks, that, after having been exposed to a cold equal to 10° of Fahrenheit's scale, the proportion of real acid was to the water as 26·6 to 34·9.[†] This proportion, as Dr. T. observes, is probably excessive; but it must be noted, that the means employed by Berthollet were not adequate to the condensation of water held in the elastic form by an affinity so powerful as that between the muriatic acid and its water appears to be. Dr. Thomson considers the proportion of 25 per cent. given by M. M. Gay-Lussac and Thenard [‡], as being probably near the truth. Mr. Dalton is, however, inclined to believe that muriatic acid gas contains no aqueous vapour; and the reasoning employed by him is to the following effect.[§] If oxygen, hydrogen, nitrogen, or any of the other gases which are not readily, and in considerable quantity, absorbed by water, be brought into contact with that fluid, the vaporific force of the heat to which they may be exposed will raise a portion of it into the interstices of their particles; but if fluoric, muriatic, sulphuric, or nitric acid, in the state of gas, be placed in similar circumstances, an attraction is exerted between it and the water; in consequence of which the acid assumes the liquid form. "Hence," adds Mr. Dalton, "it should seem that these acid gases, so far from obstinately retaining their vapour, as is commonly imagined, cannot be induced to admit any vapour at all, in ordinary circumstances." This reasoning is very plausible, and certainly highly ingenious; but it seems to me not unobjectionable. The gas experimented on by Mr. Dalton was of necessity previously saturated with aqueous vapour; and, therefore, could not admit into its constitution an additional quantity. No one would hazard the assertion that muriate of lime contains no water of crystallization, assigning as a reason, that, if a very small portion of water be added to the crystals of this salt they assume the liquid form. Yet it appears to me that this assertion and proof would be exactly analogous to those offered by Mr. Dalton.

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[*] Vol. v. p. 778.

[†] Troisième Suite des Recherches sur les Lois de l'Affinité, p. 103, &c.

[‡] Recherches Physico-Chimiques, tome ii. p. 119, 123.

[§] New System of Chemical Philosophy, p. 282.

Since, then, unless we adopt the recent views of Sir H. Davy, his experiments,[*] in conjunction with those of Dr. Henry,[†] M. M. Gay-Lussac and Thenard,[‡] M. Berthollet,[§] Mr. Murray,[‖] and Dr. Bostock,[**] may be looked on as proving satisfactorily the existence of water in muriatic acid gas; let us examine the relation, in regard to quantity, subsisting between it and the oxygen, which oxymuriatic gas used to be supposed to contain.

According to the analysis of Chenevix,[††] oxymuriatic gas consists of 77·5 of muriatic acid, united to 22·5 of oxygen by weight. If to this compound a quantity of hydrogen be added, and if the mixture be then exposed to the sun's light, muriatic acid is produced. In Mr. Dalton's detail of his experiments on this subject (which are by far the most complete set that have fallen under my notice), he mentions that he always employed a quantity of water to condense the residual muriatic acid: he could not, of course, easily ascertain whether or not any water was evolved along with the acid. By calculation, I am led to believe there was not: for 22·5 of oxygen are capable of forming, by union with 3·76479 of hydrogen, a quantity of water equal to 26·26479, which approaches singularly near to the estimate of M. M. Gay-Lussac and Thenard; and which may acquire some additional probability from the circumstance that all others who have experimented on the quantity of water in this gas, have stated it as being greater than that which is assigned by these chemists.[‡‡]

Let us pursue the subject a little farther. Mr. Dalton, speaking of the quantity of hydrogen necessary to decompose oxymuriatic gas, says, "From the mean of five experiments, I am induced to conclude, that 100 measures of hydrogen require 94 measures of oxymuriatic acid gas. In every one of the experiments the acid was less than the hydrogen."[§§] I have already stated that 3·76479 of hydrogen are sufficient to unite with the oxygen which 100 of oxymuriatic gas has been thought to contain. Now 3·76479 grs. of hydrogen occupy, according to the estimate of Kirwan,* nearly 144·04 cubic inches, and 100 grs. of oxymuriatic gas, as appears from Mr. Dalton's experiments,† are equal to 137·9 inches, which, being reduced, give a proportional bulk, as 100 of hydrogen to 95·7 of oxymuriatic gas. Thus we see that Mr. Dalton's experiments agree almost exactly with the result of calculation; and if the hydrogen he employed was contaminated by the slightest admixture of extraneous matter, the quantity of oxymuriatic gas condensed by it must of course have been proportionally lessened.

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[*] Phil. Trans. 1809, p. 92, 458.

[†] Nicholson's Journal, 4to. vol. iv. p. 247.

[‡] Rech. Phys. Chim. tome ii. p. 94, &c.

[§] Journal de Physique, tome lxiv. p. 196.

[‖] Nicholson's Journal, vol. xxxi. p. 123, &c.

[**] Nicholson's Journal, vol. xxxii.

[††] See Thomson's Syst. vol. ii. p. 257.

[‡‡] It may not be unworthy of notice that 1 cubic inch of hydrogen, by union with very nearly 2 inches of oxygen, forms a quantity of water, whose weight is to that of the real muriatic acid contained in 2 inches of the acid gas, as 26 to 100.

[§§] New System, p. 307.

Mr. Dalton, having denied the existence of aqueous vapour in muriatic acid gas, ascribes the appearance of hydrogen during the action of certain bodies on that gas to the decomposition of the acid, which he looks on as a quaternate compound of oxygen and hydrogen. While, however, he admits the fact as stated by Sir H. Davy,‡ that "when potassium was heated in muriatic acid gas as dry as it could be obtained by common chemical means, the gas wholly disappeared, and from one-third to one-fourth of its volume of hydrogen was evolved, and muriate of potash was formed." He questions the accuracy of an experiment detailed in the Bakerian lecture for 1809, by which this conclusion was established. In this experiment it was found that 8 grs. of potassium absorbed 22 cubic inches of muriatic acid gas, and gave out 8 inches of hydrogen.

Before proceeding to examine Mr. Dalton's reasoning on this experiment, it may be well to take a view of it, unconnected with his speculations on the nature of muriatic acid.

According to the analysis of Sir H. Davy,§ 8 grs. of potassium may be converted into potash, by union with about 1·29152 grs. of oxygen. In order to furnish this oxygen, if water be its source, 1·495934 grs. of that fluid must suffer decomposition; and this quantity is sufficient, at the rate of 25 per cent. to give the gaseous form to 5·983736 grs. or in the proportion of 26·26479 to 5·6459 grs. only of muriatic acid gas. At the same time that the oxygen of this water is absorbed by the potassium, its hydrogen, amounting to ·204414 of a gr. or 7·82297 inches, must be set at liberty. Thus it appears that the hydrogen evolved was almost exactly equal in quantity to what could be afforded by a portion of water sufficient to oxydate the potassium employed.

The experiment appears, however, to be inaccurate, with regard to the absorption of the 22 inches of muriatic acid gas: for 9·29152 grs. of potash are capable of combining with the real acid contained in 5·59055 grs. of muriatic acid gas, which measure 9·641375 cubic inches only. And if we suppose that none of the acid condensed in consequence of the loss of its water entered into union with the potash, and that the acid thus employed carried all its water with it, we have only in addition to this quantity either 5·983736 grs. measuring 10·0062 inches, or 5·6459 grs. measuring 9·42625 inches, making in all a condensation of either 19·643675 or 19·067625 inches. But it is obviously more natural to suppose that the acid deprived of its elasticity from the loss of its water afterwards entered into union with the potash: in which case the results of the experiment should have been 13·76396 grs. of muriate of potash, an evolution of 7·82297 inches of hydrogen, a condensation of 1·328152 grs. of an-hydrous muriatic acid, and not less than 11·9977 cubic inches of residual muriatic acid gas.

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* Essay on Phlogiston, p. 30.

† New System, p. 297.

‡ Phil. Trans. 1808, p. 343.

§ Phil. Trans. 1808, p. 28.

It will be seen by a reference to page 290 of Mr. Dalton's work, that he infers by calculation that during the oxydation of a quantity of potassium sufficient when in the state of potash to unite with 22 inches of muriatic acid gas, were water the source of the oxygen, nearly 16 inches of hydrogen would be evolved; while if the acid gas suffered decomposition 8 inches would appear. This, however, is not perfectly accurate: for the quantity of real acid contained in 22 inches of the gas is capable of combining with 23·0872 grs. of potash, and, for the formation of this, 2·8175 grs. of oxygen are requisite. Now this quantity of oxygen is, in the state of water, combined with not less than 18·0444 inches of hydrogen. On the other hand, if Mr. Dalton's view of the nature and constitution of muriatic acid be correct, and if this body were the source of the oxygen, there should have been an evolution of 6·0148 inches only, from the oxydation of 20·2697 grs. of potassium. If, however, the 22 inches of muriatic acid gas were the sole source of the oxygen, 16·917 grs. only of potassium would have been required; and the evolution of hydrogen could not have exceeded 4·3739 inches. As, however, the quantity of oxygen required by 8 grs. of potassium is almost exactly sufficient to form water with 8 cubic inches of hydrogen, and as it is certain that 9·29152 grs. of potash cannot form muriate of potash by union with the real acid of 22 cubic inches of muriatic acid gas, is it not better to argue upon a foundation in some degree known, and from phenomena consistent among themselves, than to build speculations on a part of an experiment, which is almost demonstrably impossible?

In the only other experiment of this sort detailed by Sir H. Davy, 5 grs. of potassium took nearly 14 inches of muriatic acid gas, and gave about 5 inches of hydrogen: 5 grs. of potassium may unite with ·8072 of a grain of oxygen; which in the state of water hold in union ·13505 of a grain, or 5·115 inches of hydrogen. Now ·94225 of a grain of water are capable of giving the gaseous form to either 3·769 grs. (measuring 6·3027 inches) or 3·55609 grs. (measuring 5·94564 inches) of muriatic acid gas; and 5·8072 grs. of potash can unite with the real acid contained in 2·9045 grs. or 4·857 inches of muriatic acid gas: so that the condensation could at most amount to only either 11·1577, or 10·80364 inches. If the condensations were not independent of each other, the results ought to have been 8·7117 grs. of muriate of potash, 5·115 inches of hydrogen, ·8645 of a grain of an-hydrous muriatic acid, and 7·6973 inches of residual muriatic acid gas.

Mr. Dalton has, in a subsequent part of his work,* joined with the French chemists in supposing potassium to be a hydruret of potash. He does not, however, look back to the reasoning in page 290, to inquire whether, from the action of potassium on muriatic acid gas, so much hydrogen be evolved, as might, besides the quantity ascribed to the conversion of the potassium into potash, leave a surplus to be accounted for from the decomposition of the acid. It is probable that there is not: and this probability will the better appear, if, for an instant, we admit the truth of Mr. Dalton's atomic theory. In every binate compound, the weights of the combined elements are proportional to those of their atoms. Mr. Dalton has stated the weight of the atom of hydrogen at 1, and that of potash at 42; of course, that of hydruret of potash must be 43. The amount of the hydrogen evolved in Sir H. Davy's first experiment, should therefore have been $\frac{1}{43}$ of 8 grs. or ·1860465 of a grain; a quantity less than that noted by Sir H. Davy, by ·0183675 of a grain, or about ·702 of a cubic inch; and falling short of the result of calculation on the data of the experiment, by only ·52477 of a cubic inch. It is almost superfluous to say that this slight discrepancy is not to be wondered at, in an investigation which is as yet only in its infancy. Mr. Dalton, however, says that water is a binate compound; therefore if 8 grs. of potassium contain as many particles as ·204414 of a grain of hydrogen, and if the number of particles in this be equal to that contained in 1·29152 gr. of oxygen, it follows that, in these quantities of oxygen and of potassium, there exists an equal number of particles, and potash may still be an oxide of potassium. The hydrogen which appeared may of course have come entirely from the water, without any decomposition of the acid. †

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* Page 484—486.

† I would not be understood as having any reference here to Mr. Murray's views of the nature of potassium; but merely as arguing against its being a hydruret of potash.

(*To be continued.*)

ARTICLE VI.

General Views of the Composition of Animal Fluids. By J. Berzelius, M.D. Professor of Chemistry in the College of Medicine at Stockholm.*

HAVING related to my friend Dr. Marcet some observations that I have made on the subject of animal chemistry, and being invited by him to communicate them to the Medical and Chirurgical Society, I shall, in compliance with his wish, venture to submit to the Society some of the principal results that I have obtained at different periods, prior to my visit to this country, respecting the fluids of animals. Most of these observations have been published in a more unconnected state in different works in the Swedish language;† but as they have not been translated into any other language, and as they have appeared to those who have seen them, to contain some new views, I am induced to offer them to the Society, in the hope that they will be received with indulgence.

I. *Of the Blood.*

In most of the analytical researches on blood, that of the bullock has been made the subject of experiment. I shall therefore begin with the analysis of the blood of that animal, and afterwards notice the essential points in which I have found it to differ from the human.

A. *Bullock's Blood.*

Blood may be regarded as a liquid holding a colouring matter suspended in it, but not dissolved. The first step in the process of accurate analysis should therefore be to separate the suspended matter by filtration. But this method succeeds only to a certain degree, and requires a time so considerable, that the blood undergoes spontaneous changes of composition before the separation can be completed: for notwithstanding all possible care, the colouring matter will either pass through with the fluid portion, or by adhering in masses, prevent all further percolation. Another mode is that of allowing it to subside by rest: but this also goes on with extreme slowness: the clear supernatant liquor loses its red colour but very gradually; and the colourless portion is not capable of being collected alone. The usual way of obtaining them separate is to take advantage of the coagulation of the blood, during which the fibrin enveloping the colouring substance presses out the serum. This method is indeed but very imperfect, as a large portion of the serum still remains attached to the red globules in the coagulum ; but it is the only one that we can employ.

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* From the third volume of the Medico-Chirurgical Transactions, lately published.

† In my "Förelasningar i Djurkemien, 2 vol. Stockh. 1808. And also in Afhandlingar i Fysik, Kemi och Mineralogie, 3 vol. Stockholm. 1810."

I shall first consider the crassamentum, and its two constituent parts, fibrin and colouring matter.

The Chemical Properties of Fibrin.

Fibrin is insoluble in cold water. In boiling water it curls up, and after the ebullition has continued some hours, the water acquires a milky hue, but no gaseous product appears. By this operation fibrin undergoes a species of decomposition ; the water in which it is boiled affords, by the addition of tannin, a precipitate of white and distinct flocculi, which do not cohere together by the heat, as those produced by gelatin. The evaporated liquid does not gelatinize to whatever degree it may be concentrated, and leaves a white, dry, hard, and friable residue, which is soluble in cold water, and has an agreeable taste similar to fresh broth, and totally unlike the salt and acrid flavour of the extract from muscles. Fibrin, by long boiling in water, loses its property of softening and dissolving in acetic acid.

2. In *alcohol* of specific gravity 0·81 fibrin undergoes a species of decomposition, and forms an adipocirous matter, soluble in alcohol, and precipitated by the addition of water; having often a very strong and unpleasant odour. The alcoholic solution leaves on evaporation a fat residue, which did not pre-exist in the fibrin, and which, as we shall find, is likewise formed by the action of alcohol on the colouring matter and on the albumen. Fibrin heated in alcohol retains its property of softening and dissolving in acetic acid.

3. By the action of *ether* fibrin is converted into an adipocirous mass similar to the preceding, but in much greater abundance, and having a much stronger and more disagreeable odour. We are on this account precluded from employing generally either alcohol or ether in analytical experiments on animal substances.

4. In concentrated *acetic acid*, fibrin becomes immediately soft, transparent, and with the assistance of heat is converted into a tremulous jelly. By adding water and warming it, this jelly is completely dissolved, with the evolution of a small quantity of azotic gas. The solution is colourless, of a mawkish and slightly acid taste. During its evaporation a transparent membrane appears on the surface, and after a certain degree of approximation, the gelatinous substance is again re-produced: but this gelatine has no resemblance to that formed by paste. When completely desiccated it is a transparent mass which

reddens turnsol paper, but which without a fresh addition of acetic acid is insoluble both in cold and boiling water. The solution of fibrin in acetic acid added to prussiate of potash or of ammonia, gives a white precipitate, without any separation of prussic acid. This solution will also produce a precipitate by alkalies, but it is redissolved by a small excess of the latter. Sulphuric, nitric, and muriatic acids afford likewise a precipitate with this solution: and the precipitate is composed, as we shall presently see, of fibrin and the acid employed. If the precipitate be laid on a filter and washed, a certain quantity of this acid is carried off by the water, and the remaining substance is soluble in pure water. The solution contains a neutral combination of fibrin with the mineral acid employed, which is mucous, somewhat opaline, and of an acidulous taste. An addition of acid will again precipitate it, and it thus often happens that an animal substance, that has been treated with a mineral acid and washed on the filter, gives at length a clear liquor, which becomes turbid on falling into the acid liquor that had first gone through. This phenomenon always indicates the presence of the abovementioned combination of a mineral acid with fibrin, or with albumen, which appears to possess the same chemical properties as fibrin.*

4. In weak *muriatic acid*, fibrin shrinks and gives out a small quantity of azotic gas; but scarcely any portion is dissolved even by boiling: neither does the acid liquor afford any precipitate with ammonia, or with prussiate of potash. Evaporated to dryness a brownish residue is obtained, from which potash disengages a little ammonia. Concentrated muriatic acid decomposes fibrin by coction, and produces a red or violet coloured solution.

The fibrin that has been digested with weak muriatic acid is hard and shrivelled. By washing repeatedly with water, it is at length converted into a gelatinous mass, which is perfectly soluble in tepid water. The solution powerfully reddens litmus paper, and yields a precipitate with acids as well as with alkalies. Fibrin has therefore the property of combining with muriatic acid in two proportions. The one gives a neutral combination soluble in water; the other a combination with excess of acid which is insoluble, but which is reduced to the state of the soluble compound by the action of pure water.

5. Concentrated *sulphuric acid* decomposes and carbonises fibrin. The same acid diluted with six times its weight of water, and digested with fibrin, acquires a red colour, but dissolves scarcely any thing. The fibrin that is not dissolved is a combination of it with an excess of sulphuric acid. By depriving i

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* It may be observed that the precipitate produced by nitric acid assumes a yellow colour, but has in other respects the same properties as the two others.

of this excess of acid, by means of pure water, a neutral combination is obtained, which is soluble in water, and possesses the same characters as the neutral muriate of fibrin.

6. *Nitric acid* of the specific gravity 1·25 digested with fibrin, renders it yellow, and diminishes its cohesion. The fluid becomes yellow, and the surface of the fibrin is covered with a small quantity of fat formed by the action of the acid. During this operation elastic matter is disengaged, which is azotic gas alone, and in which I could not discover the smallest portion of nitrous gas. When the digestion has been continued 24 hours, the fibrin is converted into a pulverulent mass, of a pale citron colour, which is deposited at the bottom of the liquor. The latter being decanted off, and the undissolved matter placed on a filter and washed with a large quantity of water, the colour changes in proportion as the acid which was in excess is carried off, and the mass acquires a deep orange colour. Even when the affusion is continued till the water gives no sign of acidity, the orange mass has not yet lost the property of reddening litmus.

This yellow substance was discovered by Messrs. Fourcroy and Vauquelin, who obtained it in treating muscular flesh with nitric acid. They have described it as a new acid formed by the action of nitric acid on the muscular fibre, and to which, from its colour, they have given the name of yellow acid. (*Acide jaune.*) This substance is dissolved in caustic alkali, to which it imparts an orange colour, and it is partly soluble in acetate of potash and of soda. The French chemists found that, if treated with a fresh quantity of nitric acid, it acquires the property of burning with the same phenomena as a combustible body mixed with nitre. This fact they considered as remarkable, since they could not detect any nitric acid in the yellow acid. But the latter, as will be presently shewn, is nothing more than a combination of fibrin with nitric acid, (or in other cases, perhaps, with nitrous acid,) and also with another acid formed by the decomposition of a portion of the fibrin, a species of combination very analogous to the one already described. If the yellow substance be boiled with alcohol, a yellow adipocirous matter is taken up; but it is deposited when the liquor cools. This sebaceous matter has a great resemblance to that obtained by the action of alcohol on pure fibrin. If the yellow substance, after being thus deprived of its adipocirous portion, be digested with water and carbonate of lime, it slowly decomposes the carbonate, disengages its acid in the form of gas, and produces a yellow solution. Having separated this fluid from the undissolved portion of the yellow substance, I concentrated it to the consistence of a syrup: and then poured in alcohol, which precipitated one part, retaining the other in solution. The precipitate had all the characters of malate of lime. Dissolved in water, and decomposed by a sufficient quantity of sulphuric acid, it yielded sulphate of lime; and the acid liquor being filtered and evaporated, gave a brownish and highly acid syrup, possessing all the properties of malic acid. The portion dissolved in the alcohol was evidently a mixture of nitrate and nitrite of lime. The mixture consisting of undecomposed carbonate of lime, and the insoluble part of the yellow substance, being decomposed by diluted muriatic acid, left a yellow mass perfectly similar to the one which I had before decomposed, and possessing, like that substance, the property of reddening litmus paper. It appeared therefore that I had merely exchanged the nitric and malic, for the muriatic acid.

We thus find that fibrin enters into combination as readily with nitric acid, as with the other beforementioned acids, and that it is capable of forming two combinations, the one containing an excess of acid and having a pale yellow colour, the other neutral and of an orange hue. By digesting fibrin with nitric acid, it undergoes a species of decomposition by which malic acid is formed. This acid in conjunction with the nitric acid combines with the undecomposed fibrin. The fibrin thus united with the two acids is certainly in some degree altered, for its neutral combination with nitric acid is insoluble in water, and retains its insolubility and its yellow colour even when the nitric acid has been displaced by muriatic. On the other hand, we have seen that the precipitate thrown down by nitric acid from a solution of fibrin in acetic acid, acquires a yellow tinge, but that water in depriving it of the excess of acid, renders it gelatinous and again soluble. It follows that the fibrin, which, in the yellow body, performs the office of a saline base to the nitric acid, must be modified in a different manner from what it is in fibrin combined with the acids in the soluble combinations.

The nitric acid, in which the yellow substance has been formed, has a bright yellow colour: it holds in solution a portion of the yellow substance with a quantity of malic acid. Mixed with alkali in excess, it assumes a very dark yellowish brown colour.

7. In *caustic alkali*, fibrin increases in bulk, becomes transparent and gelatinous, and at length is completely dissolved. The solution is yellow with a shade of green. Acids occasion in it a precipitate which gradually becomes confluent. The solution of fibrin in caustic alkali is precipitated by alcohol, which by means of the excess of alkali dissolves a portion of the neutral combination of fibrin with alkali. If the aqueous alkaline solution be evaporated, a coagulum is formed towards the end of the process, probably in proportion as the alkali becomes carbonated. The action of alkali upon fibrin produces some alteration in its properties, for the precipitate thrown down by acetic acid does not dissolve by an additional quantity of acid. But

whatever be the nature of this change induced on fibrin by the alkalies, the former is by no means converted, as M. Fourcroy has alleged, into a fatty substance with which the alkali produces a saponaceous compound, nor has it even the least analogy to any species of soap. As far as I can perceive, this effect of alkalies is confined to the epidermoid textures, and parts which by long and continued boiling are converted into a similar substance.

Of the Colouring Matter and its Chemical Properties.

In order to separate, as much as possible, the colouring matter from the albumen and the salts of serum, I cut the crassamentum into very thin slices, which I placed upon blotting paper, till it had taken up all that it could absorb: after which they were dried. A portion of the crassamentum treated in that manner, was triturated with water as long as it appeared capable of acting as a solvent. This water acquired from the matter it had dissolved a brown colour of so deep a shade that it did not exhibit the least transparency when contained in a glass tube of a quarter of an inch in diameter. The fluid had a faint odour of blood, and a mawkish, saline, and highly nauseous taste.

I coagulated the solution by means of heat in a pneumatic apparatus. The mass frothed considerably, but no elastic fluid was disengaged. While yet hot, it was placed on a filter: the liquor had a red colour which it lost by cooling, and at the same time deposited a small quantity of the colouring matter. I shall revert to this liquor in the sequel.

The dark brown coagulated matter, after being carefully washed, and subjected to a powerful press, was dried at a temperature of 70° (158° Fahrenheit). It was but little contracted by the exsiccation, but had become black, hard, difficultly pulverisable, and shewed a vitreous fracture. Before the desiccation is complete, it is of a dark brown colour, has little cohesion, and forms a granulated mass: circumstances by which it may be distinguished from both fibrin and albumen.

1. The colouring matter is acted upon by *boiling water* in nearly the same way as fibrin, becoming somewhat contracted by coction; and the solution contains soda, and an animal matter perfectly analogous to that which is obtained from fibrin, but rather less in quantity: for it would appear that a portion of this substance begins to be formed from the commencement of the process of coagulation. It retains its black colour, but loses the property of softening, and dissolving in acetic acid.

2. *Alcohol* and *ether* convert the colouring matter in part, into a fatty adipocirous mass, having a very disagreeable odour.

3. In *acetic acid*, the colouring matter immediately becomes t, forming a black and tremulous jelly, which dissolves in

tepid water with the disengagement of a small quantity of azotic gas. The solution is reddish brown, and only semi-transparent. A small part of the colouring matter remains undissolved, and forms with the acid a compound of very sparing solubility.

A solution of the colouring matter mixed with acetic acid does not coagulate; but when made to boil turns black and deposits a very small quantity of its insoluble compound, without however coagulating.

The solution of the colouring matter in acetic acid is precipitated both by alkalies and by alkaline prussiates. Ammonia produces a dark brown precipitate, which, when well washed and weighed, is found to be the colouring matter unaltered, and again soluble in acetic acid. The solution after precipitation by ammonia is yellow, and deposits by evaporation a quantity of white matter, which is readily distinguished to be albumen, of which it is impossible entirely to deprive the crassamentum.

Prussiate of ammonia precipitates from the acetic solution a mass of a blackish brown colour, resembling the precipitate by pure ammonia. Both these precipitates, employed as pigments, give the same shade of a dirty brown. The prussic acid, therefore, appears to exert no action on the colouring matter of the blood; which should take place if the latter owed its colour to a ferruginous salt. The solution of colouring matter in acetic acid is precipitated by the mineral acids, and the precipitates have precisely the same characters, excepting the colour, which is brown, as those procured by the same methods from fibrin.

4. Concentrated *muriatic acid* does not dissolve the colouring matter, even when aided by digestion: a small quantity of azotic gas is disengaged, and the acid assumes a yellow tint: alkalies, however, produce in it hardly any precipitate. The undissolved portion is a compound with excess of acid, which becomes soluble in proportion as the superabundant acid is carried off. The neutral solution of colouring matter is brown, and has the same properties as that formed by acetic acid. The colouring matter boiled for a long time with muriatic acid, suffers a commencement of decomposition: some iron is taken up by the acid, and the undissolved portion is no longer soluble even by repeated washing, although in this state it retains a portion of acid, of which the water cannot deprive it.

5. The *nitric acid* produces the same effects in the colouring matter as on fibrin; the only distinction being in the colour, which in the former is invariably black.

6. *Caustic ammonia* dissolves, the colouring matter assuming a very deep brown colour. A precipitate is thrown down by acids, but not by the alkaline prussiates. The precipitate formed by acetic acid is again soluble, but only by an excess of acid.

7. In solution of *caustic fixed alkali* the colouring matter is softened, forming a brownish jelly, which is dissolved by a sufficient quantity of water. During the evaporation it coagulates in proportion as the alkali absorbs carbonic acid. The alkaline solution is precipitated by alcohol, which, however, acquires a red tinge by dissolving a small quantity of the compound formed of the colouring matter with the excess of alkali. The alkaline solution of colouring matter seen by day-light has a green colour, but appears red by candle-light. It was upon this appearance of green that Fourcroy conceived the idea that bile might be formed by boiling blood with a small quantity of water; but this green fluid, as we shall afterwards see, agrees with bile in no property excepting colour.

8. If a solution of the colouring matter in water be exposed to a heat of 50° (122° Fahrenheit) in a saucer, it blackens and dries completely without coagulating. In this state it is again entirely soluble in cold water.

These experiments prove that the colouring matter has the same chemical properties, and consequently the same chemical composition as fibrin, but that these two bodies are distinguishable from one another principally by a difference in colour; by the fibrin coagulating spontaneously in all temperatures, while the colouring matter may be dried, without losing its solubility in water, and becomes insoluble only at a certain temperature: and lastly by the peculiar character of the latter when coagulated, such as its not being diminished in volume during the exsiccation, as happens with fibrin.

(*To be continued.*)

Article VII.

On the Heat evolved during Inflammation of the Human Body.

By Thomas Thomson, M.D. F.R.S.

That the heat evolved by the human body is very considerable, and that in cases of inflammation this heat is very much increased, are facts with which every body is acquainted; but I am ignorant of any attempt hitherto made to estimate the increase of heat that is given off in cases of inflammation. On that account I think it worth while to record an observation which I had an opportunity of making upon myself, during the course of last winter. It is far from determining the whole heat given off during the inflammation; but as it is at least an approach towards accuracy, and as I was at as much pains as possible, considering the situation in which I was at the time, I conceive the statement will add another and a curious fact to animal physiology.

During the month of January last, in consequence of walking about in rainy weather in thin shoes for a considerable part of the day, and afterwards sitting for several hours with wet feet, I caught a violent cold, which was attended with fever, and among other inflammatory symptoms a throbbing pain took place in the right groin, accompanied with swelling of the inguinal glands. To prevent this pain from proceeding to suppuration, I applied, for four days successively, and 36 times each day, two cotton cloths successively wrung out of cold water to the swelled part. The average temperature of the cold water employed was 40°. The cloths were removed when they felt hot; and from several trials this appeared to indicate a temperature of about 90°; so that each cloth, and the water which it contained, was heated at an average 50°.

The first cloth dry weighed .... 530 grains
The second 458

The first when wet weighed 1459 or 929 water + 530 cloth
The second .... 1434 or 976 + 458

I made several experiments to determine the specific heat of cotton, but found it attended with unexpected difficulty. When cotton wool is employed it is so elastic and bulky that you are obliged to use a much smaller weight of it than of the hot water with which you mix it. This occasions great inaccuracy. When cotton cloth is employed, a considerable time elapses before you can mix it properly with the water, and this occasions uncertainty. I state, therefore, the results which I obtained with considerable hesitation. The specific heat of cotton, by my trials, is 0·53, that of water being 1. I shall therefore consider it as half as great as that of water.

We may, therefore, substitute for the two cotton cloths a quantity of water equal to half the weight of each. We may say, therefore, that 2399 grains of water were heated 50 degrees 18 times a day for four days together, making a total of 30 pounds troy heated 50° in the course of four days by the inflamed part. This is nearly the same quantity of heat that would have been requisite to heat 8½ lbs. of water from the temperature of 40° to that of 212°. This amounts nearly to seven wine pints.

So that in the course of four days this small inflamed spot gave out a quantity of heat sufficient to have heated seven wine pints of water from 40° to 212°; yet the temperature was not sensibly less than that of the rest of the body at the end of the experiment. The inflammation, however, was gone, and did not again return.

Nor was this quantity of heat, considerable as it was, the whole that was evolved. Some was lost by the evaporation of the moisture from the wet cloth, which must have taken place to a certain extent, and some must have made its escape during the night, when the wet clothes were applied very irregularly, and at long intervals.

Article VIII.

Mineralogical Observations made in the Highlands of Scotland.

By James Grierson, M.D.

On the 14th of August, 1812, Mr. Jardine, civil engineer, and myself, left Edinburgh on a round by Loch Catherine and Loch Tay, principally with the view of observing the geognostic appearances of the country.

On reaching the bridge of Doune, eight miles from Stirling, we found the old red sandstone, forming nearly horizontal strata in the bottom of the river. Soon after this we observed on the road from Doune to Callander many rolled pieces of very distinct grey wacke, which led me to suspect the approach of a grey wacke country. From Callander we set out to visit the famed and interesting scenery of the pass of Leney, by which the traveller on this route enters the Grampian range. About two miles beyond Callander we found the rock through which the road is cut to be very distinct grey wacke, and traced it till we found it about half a mile farther on towards the north-west, very near the mica slate; but could not see the junction of these two rocks, or whether the clay slate intervened betwixt them. On this road, about a mile and a half from Callander (to the N. W. of it), we observed a greenstone vein, 12 or 15 feet broad, intersecting the conglomerate in a direction nearly N. E. and S. W.: it crosses the high road. A little below this, towards Callander, transition slate appears in pretty highly inclined strata, and the conglomerate resting upon it, and, we thought, alternately with it. Neither of us could observe any instance of the conglomerate coming in contact with the clay slate; and we were both perfectly satisfied that in this district the transition rocks, grey wacke, and grey wacke slate, come in between the floetz and the primitive country.

It was my intention to have examined particularly the species of minerals constituting the nodules of the conglomerate about

Callander; but this, I am sorry to say, was not done in the manner I could have wished, having been deceived by the expectation of meeting with enough of conglomerate for that purpose next day on our way to Loch Catherine. In as far, however, as I did examine the nodules, I found them to consist principally of white quartz; but met also with a few nodules of clay slate and hornstone porphyry.

In the evening of the 16th we visited the fine fall at Bracklin bridge, about a mile to the east of Callander. The rock is conglomerate, and broken down by the action of the water into many fine and fantastic forms. We had the conglomerate all along from Callander to this fall; and on tracing the river about two miles up observed no other rock: but Mr. Jardine told me that some time ago Sir James Hall found grey wacke about a mile or two higher up than we were. Night prevented our reaching it.

The grey wacke and grey wacke slate appear about two miles after leaving Callander, on the road to Loch Catherine. They continue all along the valley of Loch Venachar and Loch Achray to the Trossacks, and the eastern part of these hills is composed of grey wacke. The western part, next to Loch Catherine, is mica slate; and this is the rock all along the banks of the lake to its top. The Lady's Isle, rendered now so famous by the poetry of Mr. Scott, is a roundish mass of mica slate, about 100 yards diameter, projecting finely from the surface of the water, and covered with wood. On the Isle of Pontenellan, very near the head of the lake, we found a large rolled mass (about two feet diameter) of very beautiful primitive greenstone; but saw no rock in situ except the mica slate.

About half way between Callander and the Trossacks, that is, five miles from each, is a quarry of clay slate. It is on the right hand of the road as we go toward the Trossacks, on the banks of Loch Venachar, and 100 feet at least above the level of the lake. I examined it; but could not see the junction of this slate with the grey wacke or the mica slate. The strata are highly inclined, between 70° and 80°, and dip towards the hill. Having no compass (as we had unfortunately brought one with us which was good for nothing) I could not ascertain their direction, but it is somewhere between E. and N. E.

At Stewart's House, two miles from the foot of Loch Catherine, we have the grey wacke; and about 150 yards up the hill, towards the N. in the bed of the rivulet, appears slate which I took to be transition. The hill rises rapidly, and in less than 100 yards more we have the mica slate.

On leaving Stewart's we proceeded over the hills to the head of Loch Lubnaig, a walk of nine miles. In this route we crossed Glenfinlass, the mica slate formation accompanying us all the way; and when we had passed the top of the mountain, immediately to the west of Loch Lubnaig, we found a bed of rather dark greyish blue foliated limestone. There is a quarry in it, which has been lately wrought.

About half way between Loch Lubnaig and Loch Ern Head I found a considerable large rolled piece of the rock composed of felspar and dialage. On this road I observed many rolled pieces of primitive greenstone. The formation is all along the mica slate.

Two miles down from Loch Ern Head, on the north bank of the lake, is at present worked a large quarry of greyish blue foliated limestone. It is a bed in the mica slate, and is about 40 feet thick, and for a considerable way worked completely to the bottom. It belongs to Lord Breadalbane, and is carried down the lake in boats of 15 tons burden. It is carted from the lower end of the lake into the country about Comrie, Crieff, and other places in Strathern where the coals are to be had, and there burned for use.

The whole of the route from Loch Ern Head to Killin is the mica slate. We found, however, a bed of greenstone, or a vein, for we could not certainly determine which, seemingly of great thickness, by estimation 40 or 50 yards. It is about five miles from Loch Ern Head, and is seen in a small rivulet, where there is a bridge; and this rivulet (over which the bridge is thrown), about 200 yards to the west, falls into another rivulet, which comes from a small lake in sight of the road. This is about the highest ground over which the high road passes between Loch Ern Head and Killin. The greenstone is best seen near to where the two before-mentioned rivulets join.

On arriving at Killin we went to view the falls of the Lochy. They are over the mica slate; and we saw no other rock hereabouts. At Taymouth Lord Breadalbane has built a new house, or castle, in the Gothic style, of a fine greyish green slaty stone, so soft as to be easily wrought by the chisel; and when polished, it has nearly the same appearance as the chlorite slate, of which the castle of Inverary is built. This stone, which I believe is something intermediate between chlorite slate and talc slate, is got from a quarry not far from the Tay, about three miles to the east of Taymouth. I had no opportunity of learning with what rock or rocks it is associated.

Leaving Taymouth we proceeded to Aberfeldie, and went to see the falls of Moness. These possess more of the grand and terrific than any waterfall that I have seen in the kingdom, except the fall of Foyers. The rock is mica slate. We observed, however, a bed of greenstone, and also hornblende rock. Here we have wall precipices 200 feet high. From this place southward for 15 miles over the bleak dreary muir by

Ambleree to Glenalmond we found the same formation. This glen struck us as narrower and more confined than any we had seen, and the rocks on each side are extremely precipitous. Still mica slate strata nearly vertical; but as soon as we had got a mile, or a mile and a half, down from the bridge, we came upon transition rocks, grey wacke, and grey wacke slate. We saw also about 60 yards up the hill on our right a small quarry, in what we deemed to be clay slate. Indeed, we had no doubt of its being so, from some fragments we found on the road. On advancing some miles farther on our way towards Crieff, we again fell in with the conglomerate.

On our way from Crieff to Comrie, near the House of Lawers, on the right hand side of the road, I observed in the conglomerate a very distinct nodule of mica slate; the only one of the sort I saw in all this district. I nowhere observed any granite or gneiss nodules.

The country, looking west from Comrie, is primitive, and extremely bold. And the view up the Ern towards Duneira, a distance of nine miles, is picturesque in a high degree. On a well-chosen eminence to the north of Comrie they are erecting a monument to the memory of the late Lord Melville, of a beautiful variety of sienite, in which the felspar is almost snow white.

About three miles to the south of Comrie the road is cut through the conglomerate, in some places to the depth of 20 feet. The nodules almost all of the trap kind, amygdaloid, trap-tuff, basalt, greenstone, wacke, *grey wacke*, hornstone porphyry. From this place to the bridge of Ardoch (nine miles) the rock appeared every where to be the old red sandstone of a small grain, such as we observed it in the bottom of the river at the bridge of Doune; and nothing but sandstone presented itself till we came into Glenagles, which enters the Ochil Hills by the north, and there we found the rock to be porphyry slate. On leaving the head of Gleneagles, and descending into Glendevon we observed conglomerate lying on grey wacke and transition slate, on the north bank of the river, about three miles above the church.

The river Devon struck us as flowing all along with uncommon rapidity. It is altogether, perhaps, one of the most singular rivers in Scotland. The rapidity of its fall from its source down the glen till it arrive at the *Crook*, where, from an easterly, it changes its course to a westerly direction; and then the surprising and very extraordinary phenomena it exhibits at the Devil's Mill, the Rumbling Bridge, and Cauldron Lin, are very striking objects. At the mill and the bridge the rock is a hard conglomerate; at the Cauldron Lin it is greenstone. When proceeding from the top of the Cauldron Lin down to the bottom, by the right bank of the river, we found some men working a quarry of greenstone. This was not more than 50 or 60 yards from the lin, and we easily traced the greenstone to the lin; at first we took it for a vein; but on further examination at the bottom of the lin, finding the rock there greenstone, and seeing large and thick strata of slate clay, with some clay iron stone, alternating, and the strata of these dipping eastward, and running below the greenstone, we were both inclined to think it was a bed. From this point to the shores of the Frith of Forth the country is entirely composed of sandstone, limestone, slate clay, bituminous shale, clay iron stone, coal, and various rocks of the floetz trap series. Immediately on the shores of the Forth, these strata are in some places covered with an alluvial deposite, which contains many organic remains. These remains are in general of shells that belong to species which at present inhabit the Frith of Forth. Mr. Bald, in an excellent paper on the local fall of Alloa, inserted in the *Memoirs of the Wernerian Natural History Society*, enumerates the following species:—

1. Ostræa edulis.
2. Mytilus edulis.
3. Cardium edule.
4. Turbo littoreus.
5. Donax trunculus.
6. Patella vulgata.

Article IX.

On the Daltonian Theory of Definite Proportions in Chemical Combinations. By Thomas Thomson, M.D. F.R.S.

I promised in an early number of the *Annals of Philosophy* some observations on Mr. Dalton's theory of definite proportions, and I now sit down to fulfil that promise. Too much attention cannot be paid to this important theory, the developement of which I consider as the greatest step which chemistry has yet made as a science. It puts us in the way of establishing principles of rigid accuracy as the foundation of our reasoning, and to call in the assistance of mathematics to promote the progress of a science which has hitherto eluded the aid of that unrivalled instrument of improvement. The idea of definite proportions seems to have struck the mind of Richter, though the methods which he took to determine them were far from successful; and Mr. Higgins, in his work on phlogiston, maintained the opinion that chemical bodies unite atom to atom. But the generallization of the doctrine, and the striking and irresistible proofs deduced [illegible] the combinations of the simple substances, and the acids and bases belong entirely to Mr. Dalton; without whose labours the theory would probably have still remained unknown. On the Continent the notions originally established by Dalton have been adopted, and ingeniously extended in certain cases by Gay-Lussac; and Berzelius has published a most elaborate, extensive, and accurate set of experiments on the same subject, which fully confirm the Daltonian doctrine, while he has deduced several subordinate laws from his analyses, which, though in some measure empyrical, are nevertheless of very considerable importance in determining the constitution of bodies.

In this country less attention has hitherto been bestowed upon Dalton's theory than could have been anticipated from the sagacity and enlightened views of many of our chemists. Dr. Bostock has even written an essay against it; but from the well-known candour and liberality of this able philosopher, I have no doubt that he will embrace it with ardour as soon as his doubts are removed. Sir Humphry Davy has embraced the Daltonian theory with some modifications and alterations of terms; but his notions are not quite so perspicuous as those of Mr. Dalton, and they do not appear to me so agreeable to the principles of sound philosophy. These, as far as I recollect, are the only chemists in this country who have written upon the subject (some observations of Dr. Wollaston, and of myself, excepted); though not the only persons who have embraced the opinions of Mr. Dalton. I shall in this essay state, in the first place, the nature of the theory, and the grounds upon which it has been advanced; in the second place, I shall state the laws, or subordinate canons, which have been deduced from analysis, in consequence of the introduction of this theory; and in the third place, I shall give a table of the numbers representing the proportions in which substances combine, deduced from the application of the Daltonian theory to the most accurate analyses hitherto made.

I. *Outline of the Daltonian Theory.*

It may be necessary to mention in the outset that I propose to give the view which I have been accustomed myself to take of the subject, and that I would not be understood to make Mr. Dalton answerable for the opinions which I shall state. I call it the Daltonian theory because I consider it as belonging to Mr. Dalton; because he first suggested it to me, and set me to think on the subject; and, of course, every thing here stated originated from him, either directly, or at least indirectly.

1. With respect to the nature of the ultimate elements of bodies, we have no means of obtaining accurate information; but it is the general opinion that they consist of *atoms*, or minute *solids*, incapable of farther division. That these *atoms* are mere mathematical points surrounded with spheres of attraction and

repulsion, as Buscovich supposed, appears to me incomprehensible. They must, I think, be physical points, as minute as you will, but still possessed of length, breadth, and thickness. This opinion, I say, is generally received by philosophers; and I cannot, for my part, conceive any other. It is taken for granted as the foundation of the Daltonian theory; and, I presume, will be readily admitted by every one without hesitation.

2. In cases of the chemical union of one body with another, the substances combined are dispersed every where through the whole mass. Thus chalk is a compound of lime and carbonic acid. Now how minute a portion soever of chalk we take, we shall find it to contain both lime and carbonic acid. How minute a portion soever of water we take, we shall find it to contain both oxygen and hydrogen. How minute a portion soever of saltpetre we examine, we shall find it to contain both nitric acid and potash. Now this could not be the case unless the atoms of the combining bodies united with each other. This accordingly is the opinion universally entertained respecting chemical combinations. It has been long generally admitted, and does not therefore require any farther illustration.

3. All chemical compounds contain the same constant proportion of constituents with the most rigid accuracy, no variation whatever ever taking place. Water is universally composed of 1 part of hydrogen and 7·5 parts of oxygen; sulphuric acid, of 1 part of sulphur and 1·5 part of oxygen; carbonic acid, of 1 part of carbon and 2·7 parts nearly of oxygen, by weight. This permanency of chemical compounds is generally admitted. Indeed, the whole science of chemistry is founded on it, and depends upon it. Even Berthollet, who contends for indefinite proportions in the abstract, admits the incontrovertible fact that the proportions of chemical combinations in general are permanent.

4. This permanency of chemical compounds cannot be owing to any thing else than to the union of a certain determinate number of the atoms of one constituent with a certain determinate number of the atoms of the other. Let us suppose water the compound. Let the number of atoms of oxygen which unite be x, and of hydrogen y, then an integrant particle of water will in every case be $x + y$.

5. Oxygen has the property of uniting with different bases in various proportions, sometimes in two, sometimes in three, four, or even six proportions with the same base. Thus with azote it unites in four proportions, with carbon in two, with mercury in two, and so on. Now if we represent the weight of base with which the oxygen unites by a, and suppose all the different proportions of oxygen to unite with this portion of base; and if we denote the first portion of oxygen by b; then, in general, the

constituents of the different compounds formed by the union of the different doses of oxygen with the base will be as follows:—

1st compound, $a + b$
2d compound, $a + 2b$
3d compound, $a + 3b$
4th compound, $a + 4b$

Suppose 10 parts of oxygen enter into the first compound, then 20 parts enter into the second compound, 30 parts into the third compound, and 40 parts into the fourth compound. Hence, whatever number of atoms of oxygen enter into the first compound, twice that number enters into the second, thrice into the third, and four times that number into the fourth.

Hence it is clear that there is a determinate number of atoms of oxygen which always enter into these combinations. If we represent this number by x, then $a+x$ is the first compound, $a+2x$ the second, $a+3x$ the third, and $a+4x$ the fourth. Now it would be singular if 2, 3, 4, &c. atoms of oxygen were to be always inseparately linked together, so as never to be able to enter into combinations separate. It is much more simple to conceive that x represents only one atom. Even though the opinion should not be mathematically true, still it would be proper to adopt it: for as far as our calculations are concerned, a number of atoms of oxygen constantly and invariably united constitute a compound atom, about which we may reason as accurately and justly as we could about the simple atoms themselves. Indeed, I think that x certainly represents one atom only; for oxygen gas being a permanently elastic fluid, must consist of atoms that repel each other. Hence I conceive that a compound atom of oxygen, or a number of atoms of it united together, is impossible. And if x consisted of atoms not united together, I can see no reason why the same number should unite in every case (or a multiple of it) with other bodies.

This reasoning may be applied to hydrogen as well as oxygen. Hydrogen has the property of uniting in different proportions with various bodies, as with carbon, phosphorus, sulphur, &c. In these different proportions we find the hydrogen always denoted by y, $2y$, &c. Hence we have every reason to conclude that y, which represents the proportion of hydrogen which unites with the other constituent in these cases is an *atom*.

The numbers x and y are easily discovered, by making an accurate analysis of the different compounds into which various proportions of oxygen and hydrogen enter, and when reduced to their lowest terms they are very nearly $x=7{\cdot}5$ and $y=1$. Hence these numbers represent the ratios of the weight of an atom of oxygen and an atom of hydrogen to each other. Now it deserves attention that these numbers represent the composition of water.

For it has been ascertained, I think, with precision, that water is composed of 100 measures of oxygen gas, and 200 measures of hydrogen gas. Now the specific gravity of these gases are as follows:—

Oxygen	1·104
Hydrogen	0·073

Hence water is composed by weight of

Oxygen	7·56
Hydrogen	1·00

From this coincidence we are entitled to conclude that water is formed by the union of an atom of oxygen to an atom of hydrogen. This very important conclusion is supported by other considerations. Oxygen and hydrogen have never been made to combine in any other proportion than that in which they exist in water. Hence this proportion must be that which unites most readily, and with the greatest force. Now as the atoms of hydrogen repel each other, as is the case also with the atoms of oxygen; and as hydrogen is attracted by oxygen; it is obvious that when they are mixed equably, as is the case when 200 measures of hydrogen gas, and 100 measures of oxygen gas, are put into a tube, and fired by electricity, they will most readily unite atom to atom. This, though not in itself decisive, is a corroborating circumstance. It follows from it that a given bulk of hydrogen gas contains only one-half the number of atoms that exist in the same bulk of oxygen gas.

6. Knowing the weight of an atom of oxygen and of an atom of hydrogen, we have it in our power to determine the weight of an atom of the other substances which unite with oxygen, or with hydrogen, or with both. For example, 100 parts of sulphur unite with two proportions of oxygen, the first consisting of 100, the second of 150 parts, both in weight. Here the proportions of oxygen being to each other as the numbers 1, 1½, or as 2, 3, it is reasonable to suppose that the first portion represents two atoms of oxygen, and the second three atoms; and that there is another compound, consisting of sulphur united with one atom of oxygen, not yet discovered. If this supposition be reasonable, it follows, that the weight of sulphur which enters into these combinations represents an atom of that substance. Therefore 100 represents an atom of sulphur, and 100 two atoms of oxygen; so that an atom of sulphur, it appears, is just double the weight of an atom of oxygen.

We have it in our power to verify this reasoning, by means of the combinations which sulphur makes with hydrogen. It has been ascertained that 100 measures of hydrogen gas, when they unite with sulphur, do not alter their bulk, but merely their specific gravity. Hence, in order to determine the constituents of sulphureted hydrogen gas with perfect accuracy, we have only to ascertain correctly the specific gravity of hydrogen gas and of sulphureted hydrogen gas. Now

100 cubic inches of hydrogen gas weigh	2·230 gr.
100 cubic inches of sulphureted hydrogen ...	35·890

Hence it follows that sulphureted hydrogen gas is composed of

Hydrogen	2·23 or	1·00
Sulphur	33·66	15·09

This shows us that if sulphureted hydrogen gas is composed of an atom of hydrogen united to an atom of sulphur (and hardly any other supposition seems admissible), then if an atom of hydrogen weigh 1, an atom of sulphur will weigh 15·09. The combination of oxygen and sulphur gave us 15·12 for the weight of an atom of sulphur. Thus the two processes of reasoning lead to the same conclusion, since the difference between 15·00 and 15·12 is only $\frac{3}{1000}$. This is as near a coincidence as it is possible to obtain from chemical experiments, where absolute precision, from the nature of our processes, is impossible.

By a similar mode of reasoning, we may determine with considerable accuracy the weight of an atom of azote, phosphorus, carbon, and the metals. It would be tedious to state the methods here at full length; but some of the most important of them will be given afterwards.

It is hardly necessary to observe, how very powerfully a particular conclusion is confirmed when we arrive at it by different processes. This advantage we have in full perfection when we set about determining the weight of the atoms of the simple substances. In most cases we come to the same conclusion by two, three, or four different methods. These coincidences, I think, could not exist, unless the conclusion were well founded.

7. I shall terminate this part of the subject with Mr. Dalton's canons for the combination of the atoms of bodies with each other. They are very ingeniously contrived, and their truth, I conceive, will be readily admitted by every person who pays due attention to the subject:—

1st. When only one combination of two bodies can be obtained, it must be presumed to be a *binary** one, unless some cause appear to the contrary.

2d. When two combinations are observed, they must be presumed to be a *binary* and a *ternary*.

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* By *binary*, Mr. Dalton means a compound of one atom of one body with one atom of another; by *ternary*, a compound of one atom of one body with two atoms of another; and so on.

3d. When three combinations are observed, we may expect one to be *binary*, and the other two *ternary*.

4th. When four combinations are observed, we should expect one *binary*, two *ternary*, and one *quaternary*, &c.

5th. A *binary* compound should always be specifically heavier than the mere mixture of its two ingredients.

6th. A *ternary* compound should be specifically heavier than the mixture of a binary and a simple, which would, if combined, constitute it, &c.

7th. The above rules and observations equally apply, when two bodies, such as C and D, D and E, &c. are combined.*

If the observations of Gay-Lussac be correct, nitrous gas constitutes an exception to Mr. Dalton's 5th rule. It will come under our examination hereafter.

II. *Chemical Canons founded on the above Theory, but deduced from Analysis.*

1. When gaseous bodies combine they always unite in determinate proportions; and if we represent the bulk of the gas that enters into the compound in the smallest quantity in bulk by 1, then the bulk of the other constituent is either 1, 2, or 3. Thus muriate of ammonia is composed of 1 muriatic acid + 1 ammonia in bulk; carbonate of ammonia, of 1 carbonic acid + 1 ammonia; nitrous gas, of 1 azote + 1 oxygen; water, of 1 oxygen + 2 hydrogen; gaseous oxide of azote, of 1 oxygen + 2 azote; nitrous acid, of 1 azote + 2 oxygen, or of 1 oxygen + 2 nitrous gas; sulphuric acid, of 1 oxygen + 2 sulphurous acid; carbonic acid, of 1 oxygen + 2 oxide of carbon; ammonia, of 1 azote + 3 hydrogen; nitrous acid gas, of 1 oxygen + 3 nitrous gas. This canon has been established by Gay-Lussac, I think, in a satisfactory manner.† The only one of his conclusions which is still doubtful is that nitrous acid is a compound of 2 nitrous gas + 1 oxygen gas. At least I have not been able to make the two gases unite exactly in that proportion. This canon is obviously connected with the Daltonian theory. It is simple and beautiful, and of considerable utility in practical chemistry.

2. The quantity of acid requisite to saturate the different metals is directly as the quantity of oxygen which these metals require to convert them into oxides. Thus 100 parts of mercury require 4·16 parts of oxygen, and 100 parts of silver require 7·9 parts of oxygen, to convert them into oxides. Therefore the quantity of acid necessary to saturate 100 parts of mercury

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* Dalton's New System of Chemistry, vol. i. p. 214.

† Mem. d'Arcueil, vol. ii. p. 207.

is to the quantity necessary to saturate 100 parts of silver as the number 4·16 to 7·9. This law was first pointed out by Gay-Lussac.* It may be expressed in the following manner, which adapts it better to the purposes of the chemist. When different metallic oxides saturate the same weight of acid, each contains exactly the same weight of oxygen. According to Berzelius, in order to saturate 100 parts of muriatic acid, a metal must be combined with 42 parts of oxygen; to saturate 100 of sulphuric acid, it must be combined with 20 parts of oxygen.

I believe that this law applies only to those metals which are precipitated by each other, namely, gold, silver, mercury, copper, lead, cobalt, and perhaps iron, zinc, and one or two others. The other metals, I conceive, follow a different law; and it is because they follow a different law that they are not precipitated. This will appear more obviously hereafter, when we come to examine the constitution of the metallic salts.

3. When sulphur combines with a metal, the proportion remains unchanged, though the sulphur be converted into an acid, and the metal into an oxide. Thus the proportion of metal and sulphur in sulphate of copper is the same as in sulphuret of copper. Hence sulphuret of lead, when treated with nitric acid, is converted into neutral sulphate of lead, sulphuret of antimony into sulphate of antimony, and so on. This law, which is of great importance in practical chemistry, and very much facilitates the analysis of the metalline salts, was first pointed out by Berzelius.

4. The oxygen in a metallic protoxide is equal to half the sulphur in the sulphuret of the same metal, supposing the weight of the metal in both cases 100. This canon was first specified by Berzelius. It depends obviously upon the fact above established, that an atom of sulphur is twice the weight of an atom of oxygen, and holds only in those cases where the protoxide is a compound of one atom of metal, and one atom of oxygen; and the sulphuret, of one atom of metal, and one atom of sulphur. It may hold also when the oxide contains two atoms of oxygen, and the sulphuret two atoms of sulphur. This is the case with the black oxide of iron and magnetic pyrites. Hence the canon is of some utility by enabling us the better to determine the constitution of the sulphurets; which, like the oxides, are susceptible of considerable variation.

5. In combinations of two bodies containing each a quantity of oxygen, the weight of oxygen in each body is either equal, or one contains twice, thrice, four times, &c. as far as eight times, the quantity of oxygen in the other. This law has been laid down by Berzelius;† but I must acknowledge I entertain considerable

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* Mem. d'Arcueil, ii. 157.

† Ann. de Chim. lxxxiii. 119.

doubts about its accuracy. It will be better to leave the investigation of the subject till we come to examine the composition of the different salts, in all of which the two constituents contain oxygen. If it hold it will indicate a certain regularity in the relative weights of the atoms of bodies which I have not yet observed.

6. Water is capable of combining both with acids and bases. When it unites with an acid it acts the part of a base, and contains the same quantity of oxygen that the base would contain. Therefore the least quantity of water that can combine with sulphuric acid must contain 20 parts of oxygen. Hence the strongest possible sulphuric acid is a compound of

Acid	77⅓
Water	22⅔
	100

When the water combines with a base, it acts the part of an acid, and combines in the same proportion. Hence such compounds are called *hydrates*. This canon has also been laid down by Berzelius. I must confess that I have not hitherto met with sufficient evidence of its accuracy to induce me to put much confidence in it; but we shall be able to judge better when we come to examine the constitution of the hydrates, than we can at present.

7. In combinations composed of more than two bodies containing oxygen, the oxygen of that constituent which contains the least of it is a common divisor of all the portions of oxygen found in the other bodies. This law, likewise laid down by Berzelius, evidently depends upon the kind of combination which these bases make with oxygen. If they are each combinations of one atom of base with one atom of oxygen, the quantity of oxygen present in all will be the same. If one is a protoxide, and another a deutoxide, then the one will contain double the quantity of oxygen in the other. And since oxygen always unites by atoms, it is obvious that all the quantities of oxygen will be divisible by one atom of oxygen. Hence the law.

If we were accurately acquainted with the constitution of the earths, this law would be of great importance to the mineralogist. It would enable him to distinguish between chemical combinations and mechanical mixtures. I have no doubt that it will ultimately throw a new light upon the chemical analysis of minerals; even at present it may be applied with some success, taking the imperfect knowledge that we have as a basis.

8. When two combustible bases unite they always combine in such a proportion that, when oxydized, either the quantity of

oxygen uniting with each would be the same, or the oxygen in the one would be twice, thrice, &c. that in the other. This is another law laid down by Berzelius, and obviously depends upon this fact, that the two bodies must either unite atom to atom, or a certain number of atoms of the one must combine with one atom of the other.

This law might be applied successfully to determine which of the metallic alloys are chemical combinations, and which are mechanical mixtures. For example, there can be no doubt that copper and zinc combine chemically. Now from the following table it will appear that the weight of an atom of these metals is as follows:—

Copper	8·000
Zinc	4·315

Therefore, if they unite atom to atom, brass ought to be a compound of 100 copper and 53·93 zinc. Now if any person will be at the trouble to analyse brass, he will find that this is very nearly the proportion of the ingredients.

In like manner bell-metal seems to be a compound of 5 atoms of copper to 1 atom of tin; and the metal for mirrors, of 4 atoms of copper and 1 atom of tin. If mercury unites atom to atom with tin, it ought to dissolve somewhat less than half its weight of that metal. And if the same law holds with zinc, it ought to dissolve about $\frac{1}{5}$th of its weight of that metal.

III. *Relative Weight of the Atoms of different Substances determined from Chemical Analysis.*

Before we can draw up a table of the relative weights of the atoms of bodies, we must fix upon some one whose atom shall be represented by unity. Mr. Dalton has made choice of hydrogen for that purpose, because it is the lightest of all known bodies. Sir Humphry Davy has followed his example; but he has doubled the weight of an atom of oxygen, and consequently of all other bodies, by the arbitrary supposition that water is composed of two atoms of hydrogen and one of oxygen. Dr. Wollaston and Professor Berzelius have both proposed the atom of oxygen as the most convenient unit: nor can there be any hesitation in embracing their plan. Oxygen is in fact the substance by means of which the weight of the atoms of almost all other bodies is determined. It enters into a much greater number of combinations than any other known body; hence the great advantage attending a convenient number for that body to the practical chemist.

It would remove a great deal of confusion, which is at present very conspicuous in this department of the science, if chemists would agree to represent the weight of the atoms by the same numbers. The following table is submitted to the chemical world as more convenient than the methods hitherto followed; and as the means employed in determining the numbers is every where stated, it were to be wished that they were adopted by chemists in general, as far as they are accurate. If the same numbers were steadily employed by all persons, they would soon be recollected by chemists, who would thus be able to state the composition of every compound without being obliged to refer to a book. The utility of such a recollection to the practical chemist is too obvious to be pointed out.

	Weight of an atom.
1. Oxygen	1·000
2. Hydrogen	0·132
3. Carbon	0·751 [a]
4. Azote	0·878 [b]
5. Phosphorus	1·320 [c]

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[a] This is obtained from carbonic acid, which I conceive, with Dalton, to be a compound of 2 atoms of oxygen and 1 atom of carbon. When charcoal is burned in oxygen gas, the bulk of the gas is not altered, it is merely changed into carbonic acid. Hence if from the weight of 100 cubic inches of carbonic acid gas = 46·313 grains, we subtract the weight of 100 cubic inches of oxygen gas = 33·672 grains, the remainder = 12·641 grains gives us the weight of carbon in 100 cubic inches of carbonic acid gas. This shows us that carbonic acid is composed of 27·29 parts of carbon and 72·71 of oxygen. My analysis of olefiant gas gives 0·748 for the weight of an atom of carbon, which comes very near the preceding.

[b] This number is obtained from nitrous gas, which, I think, Mr. Dalton has successfully shown to be a compound of 1 atom azote and 1 atom oxygen; and Gay-Lussac has proved it to be a compound of equal measures of azotic gas and oxygen gas. The number adopted by Sir H. Davy to represent an atom of azote when reduced to our proportions would be 0·860, and that of Mr. Dalton 0·714. Ammonia gives us a different ratio if we suppose it, with Dalton, to be a compound of 1 atom of hydrogen and 1 atom of azote. The weight of an atom of azote in that case would be 0·584; but if we consider ammonia as composed of 2 atoms of hydrogen and 1 atom of azote, then the weight of an atom of azote comes out 0·876. Hence I conceive ammonia to be a ternary compound.

[c] This number is deduced from phosphoric acid. Sir H. Davy has ascertained that the quantity of oxygen in phosphoric acid is double of that in phosphorous acid. Hence, I think,

		Weight of an atom.
6.	Sulphur	2·000
7.	Boron	

		Number of atoms,	Weight of a particle.
8.	Water, composed of	1 *o* + 1 *h*	1·132 [d]
9.	Carbonic oxide	1 *o* + 1 *c*	1·751 [e]
10.	Carbonic acid	2 *o* + 1 *c*	2·751
11.	Nitrous gas [f]	1 *o* + 1 *a*	1·878

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it follows, that phosphoric acid is a compound of 1 atom of phosphorus and 2 atoms of oxygen. Farther, it has been ascertained that when phosphorus is burnt in oxygen gas each grain of phosphorus absorbs 4½ cubic inches of oxygen. Therefore phosphoric acid is composed of 100 phosphorus and 151·52 of oxygen, and phosphorous acid of 100 phosphorus and 75·76 of oxygen: and an atom of oxygen is to an atom of phosphorus as 75·76 to 100, or as 1 to 1·320 nearly.

[d] I conceive water to be a binary compound. The opinion advanced by Sir H. Davy, that it is a ternary compound of an atom of oxygen united to 2 atoms of hydrogen, cannot, I think, be supported.

[e] It has been ascertained by accurate experiments that 100 measures of carbonic oxide for complete combustion require 50 measures of oxygen gas, and that the residue is 100 measures of carbonic acid gas. Hence it follows that the quantity of oxygen in carbonic acid is just double what exists in carbonic oxide, while the quantity of carbon in both is the same. Berthollet's objections to this conclusion are inadmissible. He seems, indeed, to have given up the point himself.

[f] The determination of the compounds into which azote and oxygen enter is attended with considerable difficulty. The reason for considering nitrous gas as a binary compound, and its analysis, have been stated before. I consider it as exactly determined. The reasons assigned by Dalton for considering nitrous oxide as a compound of 2 atoms of azote and 1 atom of oxygen, seem to me conclusive: and Gay-Lussac has shown that it contains just twice as much azote as nitrous gas, supposing the oxygen in both the same. Hence its composition must be as stated in the table. The acid analysed by Cavendish and Davy, and called nitric acid, I conceive to have been in fact nitrous acid. What Mr. Dalton calls oxy-nitric acid is true colourless nitric acid, which seems only to exist in combination with water or a base. This I infer from the recent experiments of Chevreul

		Number of atoms.	Weight of an integrant particle.
12.	Nitrous oxide	1 *o* + 2 *a*	2·756
13.	Nitrous acid	2 *o* + 1 *a*	2·878
14.	Nitric acid	3 *o* + 1 *a*	3·878
15.	Phosphorous acid	1 *o* + 1 *p*	2·320
16.	Phosphoric acid	2 *o* + 1 *p*	3·320
17.	Sulphurous acid	2 *o* + 1 *s*	4·000
18.	Sulphuric acid	3 *o* + 1 *s*	5·000
19.	Olefiant gas	1 *h* + 1 *c*	0·883 [g]
20.	Carbureted hydrogen	2 *h* + 1 *c*	1·015
21.	Ammonia	2 *h* + 1 *a*	1·142 [h]
22.	Hydrophosphoric gas	2 *h* + 1 *p*	1·584 [i]

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on the nitrites of lead. What is usually called *nitrous acid* is a compound of 1 integrant particle of nitric acid with 1 integrant particle of nitrous gas. But such a compound cannot be made to unite with bases. Yet it is obvious that nitrous acid is capable of forming permanent salts. This was shown long ago by Scheele, and still more recently by Chevreul.

[g] This corresponds exactly with my analysis of olefiant gas. My analysis of carbureted hydrogen gas does not correspond quite so well; though the difference is not greater than between the result of my analysis and that of Mr. Dalton, and others who have examined this gas. I consider the proofs adduced by Mr. Dalton respecting the composition of these two gases as conclusive. I have made too many experiments on them myself to be capable of yielding my assent to the statements of Berthollet on the subject, as given in Cuvier's account of the labours of the Institute for 1812, which will be found in a preceding number of this Journal.

[h] I have stated my reasons before for considering ammonia as a compound of 2 atoms of hydrogen and 1 atom of azote. There is still something mysterious about the composition of this substance.

[i] This composition is founded on Sir H. Davy's analysis. It will be seen from inspecting the table that the weight of an atom of phosphorus is ten times as great as the weight of an atom of hydrogen. Hence supposing this gas composed of 2 atoms hydrogen and 1 atom phosphorus, its composition by weight would be 100 hydrogen and 500 phosphorus. Now Davy found it composed of 100 hydrogen and 489·56 phosphorus. This I consider as a very near approximation, for a first analysis.

We have no data for determining the composition of phosphureted hydrogen. Davy's analysis would make it a compound

	Number of atoms.	Weight of an integrant particle.
23. Phosphureted hydrogen	$3h + 1p$	
24. Sulphureted hydrogen	$1h + 1s$	2·132[k]
25. Sulphuret of carbon	$1c + 2s$	2·751[l]
26. Carburet of phosphorus[m]	$1c + 1p$	
27. Phosphuret of sulphur	$1p + 1s$	3·320[n]
28. Potassium		5·000[o]

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of 3 atoms of hydrogen and 1 atom of phosphorus. But I can hardly believe that an atom of phosphorus can be capable of condensing so great a quantity of hydrogen. At any rate the great variation in the specific gravity of this gas demonstrates that two or more different gases are still confounded under the same name. Dalton has supposed it a compound of 1 atom of hydrogen with 1 atom of phosphorus; but this is not very likely. I suspect it to be usually mixed with hydrogen gas.

[k] Hydrogen gas is converted into sulphureted hydrogen gas without undergoing any change of volume. Hence its composition is known by subtracting 2·23 the weight of 100 cubic inches of hydrogen gas from 35·89, the weight of 100 cubic inches of sulphureted hydrogen. This gives us its constituents 2·23 hydrogen and 33·66 sulphur. Now 2·23 : 33·66 :: 0·132 : 1·9924. The fourth proportional ought to have been 2, to agree exactly with the weight of an atom of sulphur, as given in the table. The difference is only 38 ten thousandth parts, which is too inconsiderable to deserve any attention. Hence I conceive the constitution of sulphureted hydrogen to be perfectly determined.

[l] This is the compound formerly called alcohol of sulphur, and supposed to be a combination of hydrogen and sulphur. Berzelius and Marcet, as well as Thenard and Vauquelin, have determined its real nature. If the statement in the text be correct it ought to be a compound of 84·2 sulphur and 15·8 carbon. Now Berzelius and Marcet found 84·83 sulphur and 15·17 carbon, which is as near as could be expected. Hence there can be no doubt respecting the constitution of this substance.

[m] We have no data to determine the constitution of this compound, though there can be no doubt that it exists. Probably there may be 2 atoms of phosphorus in it to 1 of carbon; but I did not venture to state that opinion in the table, because phosphorus does not unite in that way in other cases.

[n] The two substances may be melted together in various proportions; but they remain liquid at the lowest temperature when 100 sulphur and 66 phosphorus are melted together. Hence I conceive that to be the most intimate combination, and have accordingly stated it in the table.

[o] From the experiments of Davy, compared with those of

	Weight of an atom.
29. Sodium	5·882 [p]
30. Barytium	8·731 [q]
31. Strontium	5·900
32. Calcium	2·620
33. Magnesium	1·368 [r]

Gay-Lussac and Thenard; I think it follows clearly that pure potash is a compound of 100 potassium and 20 oxygen. Gay-Lussac and Thenard have shown that the peroxide of potassium is composed of 100 potassium and 60 oxygen. (*Recherches Physico-Chimiques*, i. 132.) What they consider as the protoxide I believe to be a mixture of potassium and potash. These data are sufficient to constitute potash a binary compound, and the peroxide a quaternary compound. Hence an atom of potash must weigh five times an atom of oxygen.

[p] From the experiments of Davy, and of Gay-Lussac and Thenard, it appears that soda is a compound of 100 sodium and 34·1 oxygen, and the peroxide of sodium of 100 sodium and 51·1 oxygen. Now 34·1 is to 51·1 as 2 : 3. Hence it follows that soda must be a compound of 1 atom of sodium and 2 atoms of oxygen; and the peroxide of 1 atom of sodium and 3 atoms of oxygen. These data give us the weight of an atom of sodium as in the table.

[q] We have no direct data for determining the weight of an atom of barytium, calcium, and strontium. The numbers in the table have been calculated from one of the canons given in a preceding part of this essay; namely, that the portion of bases which saturate the same weight of acid contain the same weight of oxygen. Now to saturate 100 of sulphuric acid, 194 of barytes, 72·41 of lime, and 138 of strontian, are necessary. And each of these quantities must contain 20 oxygen. Hence if we suppose them to be compounds of 1 atom of base and 1 atom of oxygen we easily deduce the numbers in the table. They agree very nearly with those given already by Davy and Berzelius.

[r] This number is determined in the same way as the three last. We have not sufficient data for determining the weight of an atom of the metallic bases of the earths proper. The weight of an integrant particle of some of these earths themselves may be determined from the salts into which they enter with tolerable precision; and if we were to consider each of them as protoxides, it would be easy to determine the weight of an atom of the base of each. But I do not see any proof, or even analogy, that can lead us to consider them as protoxides. It would serve no purpose, therefore, to introduce such arbitrary numbers into the table. Perhaps the same objection will be made to the bases

		Number of atoms.	Weight of an integrant particle.
34.	Potash	$1p + 1o$	6·000
35.	Peroxide of potash	$1p + 3o$	8·000
36.	Soda	$1s + 2o$	7·882
37.	Peroxide of soda	$1s + 3o$	8·882
38.	Barytes	$1b + 1o$	9·731
39.	Strontian	$1st + 1o$	6·900
40.	Lime	$1l + 1o$	3·620
41.	Magnesia	$1m + 1o$	2·368
42.	Alumina		3·500 [s]
43.	Glucina		3·600
44.	Yttria		8·400
45.	Zirconia		5·656
46.	Silica		4·066 [t]
47.	Gold		24·968 [u]
48.	Platinum		12·161 [x]

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of the alkaline earths; but they stand upon rather better evidence, though it is not quite satisfactory.

[s] This, and the three numbers below it, though founded on the best analyses of the salts of these respective earths hitherto made, ought not to be much depended on, especially the number for yttria, which is derived from Klaproth's analysis of the carbonate of yttria. Of all the genera of salts the carbonates are the least to be trusted in such cases; because the affinity of the carbonic acid for the base is so weak that the salt cannot be dried without losing a considerable portion of its aeid.

[t] This number is derived from Mr. John Davy's analysis of silicated fluoric acid, and is not much to be depended on. I have inserted it, however, that the numbers for the earths may be complete. They will be found of considerable importance in examining the different analyses of stony bodies hitherto published.

[u] This is derived from the experiments of Berzelius on the oxides of gold. He obtained 2 oxides; the first composed of 100 gold + 4·005 oxygen; the second of 100 gold + 11·982 oxygen. Now the second of these quantities is triple the other. Hence the first is a protoxide, and the second a peroxide.

[x] This number is also derived from the experiments of Berzelius. He obtained 2 oxides of platinum; the one composed of 100 metal + 8·287 oxygen, the second of 100 metal + 16·36 oxygen. Hence we are warranted to consider the first as a protoxide. I have taken the mean of the two results, which gives 8·223 for the oxygen of the protoxide. He obtained the first oxide by precipitating fresh muriate of platinum with

	Weight of an atom.
49. Silver	12·618 [y]
50. Mercury	25·000 [z]
51. Palladium	14.204 [a]
52. Copper	8·000 [b]
53. Iron	6·666 [c]
54. Nickel	3·623 [d]
55. Tin	14·705 [e]

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caustic alkali; the second by digesting oxy-sulphate of platinum in caustic alkali. The first oxide is black, the second yellowish brown. (*Lärbok i Kemien*, ii. 421.) The results obtained by Chenevix do not differ materially from those of Berzelius.

[y] We only know one oxide of silver, which therefore I consider as a protoxide. By the experiments of Berzelius it is composed of 100 metal + 7·925 oxygen. This corresponds with the preceding analysis of Bucholz. (*Ann. de Chim.* lxxviii. 115.)

[z] This nearly agrees with the experiments of Sefstrom, as related by Berzelius. He found the protoxide a compound of 100 metal + 3·99 oxygen, the peroxide of 100 metal + 7·99 oxygen. (*Lärbok i Kemien*, ii. 347.) Fourcroy and Thenard found the protoxide composed of 100 metal + 4·16 oxygen, the peroxide of 100 metal + 8·21 oxygen. (*Mem. d'Arcueil*, ii. 168.) I have taken 4 as the intermediate proportion of oxygen in the protoxide, which must be very near the truth.

[a] Berzelius states the oxide of palladium to be composed of 100 metal + 14·08 oxygen (*Lärbok i Kemien*, ii. 405.) The appearances of the metal when dissolving in acids leads me to consider this oxide as a peroxide. If it be a protoxide the weight of an atom of palladium will be 28·408.

[b] It has been ascertained by very exact experiments that the protoxide of copper is composed of 100 metal + 12·5 oxygen, and the peroxide of 100 metal + 25 oxygen. (*Ann. de Chim.* lxxviii. 107.)

[c] We know two oxides of iron; the first composed of 100 metal + 30 oxygen, the second of 100 metal + 45 oxygen. Hence the first must be a deutoxide.

[d] We are acquainted with two oxides of nickel; the first from a mean of the experiments of Taputi, Rolhoff, and Berzelius, is composed of 100 metal + 27·6 oxygen; the second appears from Rolhoff's experiments to be composed of 100 metal + 41 oxygen. Hence the first must be a deutoxide, or a compound of 1 atom of metal with 2 atoms of oxygen. This gives us the number in the table.

[e] I am acquainted with two oxides of tin; the first composed

	Weight of an atom.
56. Lead	25·974 [f]
57. Zinc	4·315 [g]
58. Bismuth	8·994 [h]
59. Antimony	11·111 [i]

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of 100 metal + 13·6 oxygen, the second of 100 metal + 27·2 oxygen. Berzelius describes an intermediate oxide, which he calls the white oxide, and which is composed of 100 metal + 20·4 oxygen (*Lärbok i Kemien*, ii. 253.) Granting the existence of this oxide, it is evident that the first must be composed of 1 atom metal and 2 atoms oxygen, or that it is a deutoxide.

[f] We know three oxides of lead; namely, the yellow, the red, and the brown, composed of 100 metal, united with 7·7, 11·55, and 15·4 oxygen. Now these numbers are to each other as 2, 3, 4. Hence the yellow oxide must be a compound of 1 atom metal and 2 atoms oxygen; which gives us the weight of an atom of lead as in the table.

[g] In my System of Chemistry I have stated two oxides of zinc on the authority of Clement and Desormes; but from examining the salts of zinc I have satisfied myself that no such oxide as their protoxide exists. Hence I consider the white oxide of zinc as a compound of 1 atom metal and 1 atom oxygen. Berzelius found it a compound of 100 metal + 24·4 oxygen; Davy of 100 metal + 21·951 oxygen; my analysis gave me 100 metal + 23·5 oxygen, which is nearly a mean of the other two. I have chosen 23·175 as probably coming nearest the truth.

[h] Bismuth forms only one known oxide, which therefore I conceive to be a compound of 1 atom of metal and 1 atom of oxygen. By my analysis it is composed of 100 metal + 11·300 oxygen; by Lagerhjelm's, of 100 metal + 11·275 oxygen (*Berzelius Lärbok i Kemien*, ii. 175); by Mr. John Davy's, of 100 metal + 11·111 oxygen. The mean of these gives 11·229 for the oxygen. Hence the number in the table.

[i] We know two oxides of antimony. The first, according to Mr. John Davy, is a compound of 100 metal + 17·647 oxygen; according to Berzelius, of 100 metal + 18·6 oxygen: the second, according to Davy, of 26·471 oxygen + 100 metal; according to Berzelius, of 100 metal + 27·9 oxygen. Berzelius describes a third oxide, which he calls the yellow oxide, and states as a compound of 100 metal + 37·2 oxygen. (*Lärbok i Kemien*, ii. 159.) These numbers are to each other as 2, 3, 4. Hence I consider the first as a deutoxide, and that an atom of antimony is to an atom of oxygen as 100 to 9. Hence the number in the table.

		Weight of an atom.
60.	Tellurium	4·107 [k]
61.	Arsenic	6·000 [l]
62.	Cobalt	7·326 [m]
63.	Manganese	7·130 [n]
64.	Uranium	12·000 [o]
65.	Molybdenum	5·882 [p]

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[k] According to Berzelius the only oxide of tellurium known is a compound of 100 metal + 24·83 oxygen. Hence the number in the table.

[l] We know two combinations of arsenic and oxygen; the first contains 100 metal + 33·333 oxygen, the second 100 metal + 50 oxygen. These are to each other as 2 to 3; hence the first must contain 2 atoms of oxygen, the second 3. Hence the number in the table. Berzelius speaks of an oxide of arsenic composed of 100 metal + 8·333 oxygen. If it exist, an atom of arsenic will weigh 12·000. (*Ann. de Chim.* lxxxiii. 118.)

[m] We know two oxides of cobalt, the blue and the black. These, from the analysis of Rolhoff, are composed of 100 metal united with 27·3 and 40·95 of oxygen. (*Berzelius, Lärbok i Kemien*, ii. 297.) These numbers are to each other as 2 to 3. Hence the first must be a deutoxide.

[n] Berzelius describes five oxides of manganese; but his first, from the way in which he obtained it, is exceedingly doubtful. I reject it therefore. The first of the other four is green, the second olive, the third brown, and the fourth black. Their composition he states as follows:—

1	of	100 metal	+ 14·0533
2		100	+ 28·107
3		100	+ 42·16
4		100	+ 56·213

These numbers are to each other as the numbers 1, 2, 3, 4. Hence the first is a protoxide, and gives the weight of an atom of manganese as in the table. If Berzelius' first oxide really exist, we must double the weight of an atom of manganese.

[o] From Bucholz' experiments there seem to be two oxides of uranium, the black and the yellow; the first composed of about 100 metal + 8·333 oxygen, the second of 100 metal + 25 oxygen. These two numbers are to each other as 1 to 3. Hence the number in the table.

[p] We know two oxides of molybdenum, the blue and the white, composed, according to Bucholz, of 100 metal and 34 and 50 oxygen. Now these numbers are to each other as 2 to 3.

		Number of atoms.	Weight of an integrant particle.
66.	Tungsten		8·000 [q]
67.	Cerium		11·494 [r]
68.	Protoxide of gold	1 *g* + 1 *o*	25·968
69.	Peroxide of gold	1 *g* + 3 *o*	27·968
70.	Protoxide of platinum	1 *p* + 1 *o*	13·161
71.	Peroxide of platinum	1 *p* + 2 *o*	14·161
72.	Oxide of silver	1 *s* + 1 *o*	13·618
73.	Protoxide of mercury	1 *m* + 1 *o*	26·000
74.	Peroxide of mercury	1 *m* + 2 *o*	27·000
75.	Protoxide of palladium	1 *p* + 1 *o*	15·204
76.	Peroxide of palladium	1 *p* + 2 *o*	16·204
77.	Protoxide of copper	1 *c* + 1 *o*	9·000
78.	Peroxide of copper	1 *c* + 2 *o*	10·000
79.	Deutoxide of iron	1 *i* + 2 *o*	8·666
80.	Peroxide of iron	1 *i* + 3 *o*	9·666
81.	Deutoxide of nickel	1 *n* + 2 *o*	5·623
82.	Peroxide of nickel	1 *n* + 3 *o*	6·623
83.	Deutoxide of tin	1 *t* + 2 *o*	16·705
84.	Tritoxide of tin	1 *t* + 3 *o*	17·705
85.	Peroxide of tin	1 *t* + 4 *o*	18·705
86.	Deutoxide of lead	1 *l* + 2 *o*	27·974
87.	Tritoxide of lead	1 *l* + 3 *o*	28·974
88.	Peroxide of lead	1 *l* + 4 *o*	29·974
89.	Oxide of zinc	1 *z* + 1 *o*	5·315
90.	Oxide of bismuth	1 *b* + 1 *o*	9·994
91.	Deutoxide of antimony	1 *a* + 2 *o*	13·111
92.	Tritoxide of antimony	1 *a* + 3 *o*	14·111
93.	Peroxide of antimony	1 *a* + 4 *o*	15·111
94.	Oxide of tellurium	1 *t* + 1 *o*	5·107
95.	Deutoxide of arsenic	1 *a* + 2 *o*	8·000
96.	Arsenic acid	1 *a* + 3 *o*	9·000
97.	Deutoxide of cobalt	1 *c* + 2 *o*	9·326

Hence the blue is a deutoxide, and gives us the number in the table for the weight of an atom of molybdenum.

[q] This would result from Bucholz' analysis of the yellow oxide of tungsten, supposing it a deutoxide; but the number is not entitled to much confidence.

[r] According to Hisinger there are two oxides of cerium, composed of 100 metal and 17·41 and 26·115 of oxygen. Now these numbers being as 2 to 3, we must suppose the first compound a deutoxide. Hence the number in the table.

I have been obliged to omit the other metals for want of accurate experiments concerning their oxides.

		Number of atoms.	Weight of an integrant particle.
98.	Peroxide of cobalt	1 *c* + 3 *o*	10·326
99.	Protoxide of manganese	1 *m* + 1 *o*	8·130
100.	Deutoxide of manganese	1 *m* + 2 *o*	9·130
101.	Tritoxide of manganese	1 *m* + 3 *o*	10·130
102.	Peroxide of manganese	1 *m* + 4 *o*	11·130
103.	Protoxide of uranium	1 *u* + 1 *o*	13·000
104.	Peroxide of uranium	1 *u* + 3 *o*	15·000
105.	Deutoxide of molybdenum	1 *m* + 2 *o*	7·882
106.	Peroxide of molybdenum	1 *m* + 3 *o*	8·882
107.	Deutoxide of tungsten	1 *t* + 2 *o*	10·000
108.	Deutoxide of cerium	1 *c* + 2 *o*	13·494
109.	Peroxide of cerium	1 *c* + 3 *o*	14·494

But I have carried this table to as great a length as is consistent with the nature of this *Journal.* I shall therefore stop at present, and insert the rest of the table in a future number.

I presume the method of denoting the number of atoms combined will be intelligible to every reader. It is denoted by figures prefixed to the initial letters of the substances uniting together. But to preclude the possibility of mistake a plate has been engraved representing the first 41 substances in the table by Mr. Dalton's symbols. In it the number of atoms existing in each combination is represented to the eye. The figures in the plate refer to the figures prefixed to the table. The name following the figure in the table is the name of the symbol in the plate under the same figure. Thus after 36 in the table we find *soda.* The symbol under 36 in the plate represents soda; and we see that it is a compound of 1 atom of sodium and 2 atoms of oxygen.

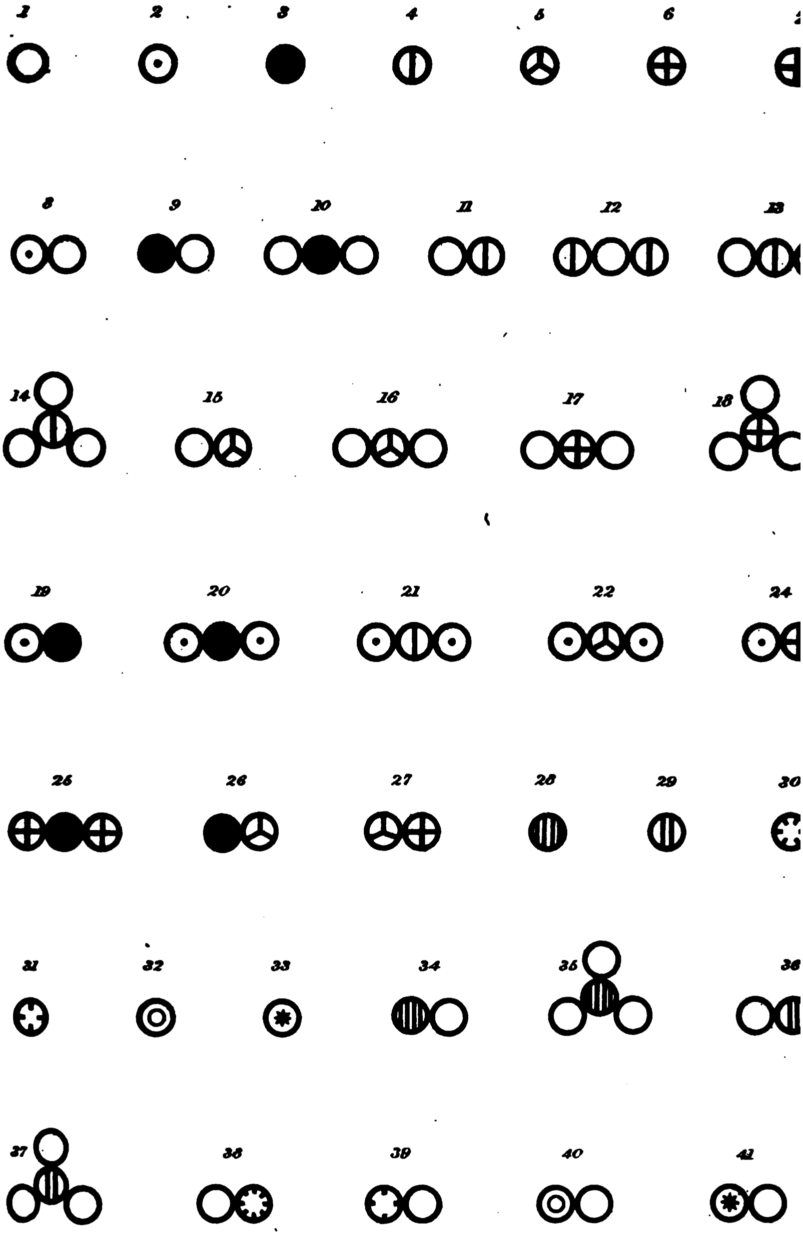
ARTICLE X.

Astronomical and Magnetical Observations at Hackney Wick.

By Col. Beaufoy.

	Hackney Wick.	Greenwich Time.
June 16, Emersion of Jupiter's 2d satellite.	$9^h\ 46'\ 39''$	$9^h\ 46'\ 45{\cdot}82''$

Observations are in Mean Time. Jupiter's limb not well defined.



SYMBOLS OF CHEMICAL BODIES.

Engraved by J. Shury for Dr. Thomsons Annals. Published by R. Baldwin, Paternoster Row. July 1. 1813.

Magnetical Observations.

Latitude 51° 32′ 40″ North. Longitude West in Time 6·82″.

1813.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
May 18	—h	—′	—°	—′	—″	1h	45′	24°	19′	20″	6h	7′	24°	14′	20″
Ditto 19	7	37	24	10	45	1	20	24	21	40	7	33	24	13	04
Ditto 20	6	55	24	11	04	1	50	24	22	16	6	58	24	15	48
Ditto 21	8	32	24	11	22	1	40	24	20	37	6	50	24	16	58
Ditto 22	8	41	24	13	59	1	30	24	20	48	—	—	—	—	—
Ditto 23	8	50	24	12	17	1	25	24	21	27	—	—	—	—	—
Ditto 24	—	—	—	—	—	—	—	—	—	—	6	17	24	16	22
Ditto 25	8	07	24	12	10	1	35	24	19	05	6	05	24	14	31
Ditto 26	8	45	24	16	15	—	—	—	—	—	7	25	24	14	54
Ditto 28	8	25	24	16	10	1	40	24	22	24	7	45	24	14	04
Ditto 29	8	10	24	14	30	1	30	24	21	42	7	22	24	14	03
Ditto 30	8	34	24	11	06	1	52	24	19	18	8	00	24	15	00

Mean of Observations in May.	Morning	at	8h 28′.....	Variation	24°	12′	02″
	Noon	at	1 37	Ditto	24	20	54
	Evening	at	6 14	Ditto	24	13	47
Ditto in April	Morning	at	8 31	Ditto	24	09	18
	Noon	at	0 59	Ditto	24	21	12
	Evening	at	5 46	Ditto	24	15	25

Magnetical Observations continued.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
June 1	—h	—′	—°	—′	—″	—h	—′	—°	—′	—″	7h	45′	24°	17′	23″
Ditto 2	8	45	24	09	30	1	15	24	20	30	7	45	24	13	09
Ditto 3	8	10	24	10	32	1	35	24	21	19	6	47	24	14	31
Ditto 5	8	52	24	13	16	1	40	24	20	39	6	50	24	13	14
Ditto 6	8	42	24	12	55	1	30	24	17	54	7	33	24	18	35
Ditto 7	8	35	24	12	02	1	42	24	21	55	6	25	24	18	03
Ditto 8	8	30	24	14	22	1	30	24	21	53	6	30	24	17	35
Ditto 9	8	15	24	14	42	1	35	24	21	15	6	20	24	16	54
Ditto 10	8	32	24	14	20	—	—	—	—	—	—	—	—	—	—
Ditto 12	8	33	24	15	55	1	42	24	23	21	6	30	24	19	19
Ditto 13	8	20	24	10	40	1	35	24	23	02	6	30	24	14	56
Ditto 14	8	22	24	12	22	1	30	24	23	30	6	35	24	15	34
Ditto 15	8	17	24	14	38	1	30	24	24	40	—	—	—	—	—
Ditto 16	8	27	24	09	30	1	42	24	23	00	7	45	24	16	16
Ditto 17	8	27	24	13	14	—	—	—	—	—	—	—	—	—	—

It may not be improper to remark, that previous to any observation the instrument is examined, and, if necessary, adjusted.

The observations are made with two needles: one of them is a cylinder, terminating at each end in a cone; the other is of the usual shape, a slender parallelopipedon, and weighs 48 grains; the weight of the former is 65 grains. These two needles, in general, give the same variation, seldom differing from each other more than one minute, and the mean of both is considered as the true variation. From the observations hitherto made, the least variation is between the hours of eight and nine in the morning; it then increases till a few minutes before two in the evening, and afterwards decreases; but at what hour the variation arrives at the same point it stood at in the morning is uncertain; but, from several observations, it is after eleven at night. It occasionally happens that the needle has a lateral vibratory motion; and, as far as I am able to judge, it happens on those days when the air has a peculiar softness; the degree of this motion does not exceed one minute and a half.

It is singular that on the 6th inst. the variation in the evening observation was greater than that at noon.

Rain fallen between the 1st of May and the 1st of June, 2,118 inches.

Article XI.

Analyses of Books.

I. *Memoirs of the Literary and Philosophical Society of Manchester. Second Series.* Vol. II. 1813, pp. 484.

(Continued from Vol. I. p. 465.)

9. *Cursory Remarks on the Mineral Substance called in Derbyshire Rotten Stone.* By William Martin, F.L.S. &c.] This mineral is considered as a variety of tripoli, and is used for the same purposes. It occurs on Bakewell Moor, in Derbyshire, and in some other places; and the pits are only opened every third or fourth year, according to the demand. It lies over a kind of limestone, containing animal remains, which Mr. Martin calls black marble; and he thinks it has been formed by the decomposition of this marble, and the abstraction of the calcareous particles. Rotten stone consists chiefly of alumina, with a little silica, and bitumen. Carbonate of lime and oxide of iron are occasional, but not constant ingredients. The following are two analyses of it by Mr. Martin:—

1. *Hard Rotten Stone, of a brown colour.*

Alumina	80
Silica	2
Carbonate of lime	10
Oxide of iron	1
Inflammable matter	5·5
Loss	1·5
	100

2. *Soft Rotten Stone.*

Alumina	87
Silica	4
Inflammable matter	7·5
Loss	1·5
	100

10. *On National Character.* By Thomas Jarrold, M.D.] The object of this ingenious essay seems to be chiefly to refute some of the notions entertained by Mr. Malthus of the effect of an increase of population beyond a certain limit. Dr. Jarrold affirms that the character of a nation neither depends upon their customs, their religion, nor the state of their industry, and that it can only be learned from history alone. His opinion seems to be that it depends upon the original character stamped upon the nation by its founder, and that this character never afterwards changes. Nothing can be more ingenious than the arguments brought forward in support of these opinions; but my old friend Dr. Jarrold must excuse me for thinking that in several particulars he has carried his consequences rather too far. Let him consider the character of the Lacedemonians, and see whether it was not entirely modelled by the customs and laws introduced by Lycurgus. Let him consider the manners and religion of the Mexicans and Peruvians, and consider whether these manners were not prodigiously influenced by their religious opinions. Let him read the history of the Dutch, and see whether that extraordinary nation has not possessed at different times three very different characters, brought on by causes that can be explained. Neither can I subscribe to the distinction of nations into honest and unwarlike, and dishonest and warlike. No nation under the sun is more dishonest than the Chinese. Consult Anson on the subject, or any person who has visited China. Yet no nation is more unwarlike. Though the speculation respecting the natural progress towards civilization is very beautiful, and highly ingenious, it will not stand the test of a

rigid examination. Read the history of the Pellew Islands, and of the conduct of Lee Boo in England. You have in him an example of a perfect savage becoming civilized, having an enlightened mind, and forming plans for the improvement of his own nation. Read the history of the Sandwich Islands, at present, for something similar in the king of that country, Peter I. of Russia appears to me another example of the same kind. As to the speculation about the taming of animals, it is equally contradicted by many facts familiar to most people. Read Cowper's account of his three hares. One was naturally tame, one became tame by kindness, one could not be tamed at all. How easy is it to make a sparrow perfectly tame, so as to follow its master for miles together upon the wing. But who has succeeded in taming a robin? I could easily make many other remarks upon this ingenious essay; but I have already gone too far considering the subject, and must therefore apologize to my readers for the length of the observations already thrown out.

11. *Observations on the Ebbing and Flowing Well at Giggleswick, in the West Riding of Yorkshire; with a Theory of reciprocating Fountains. By* Mr. John Gough, *in a Letter to* Dr. Holme.] Mr. Gough gives an account of no fewer than six wells of this kind; one in Italy, one in Greece, one in Westphalia, and three in England. The one at Giggleswick has been examined with care; and there is a table of the time, and of the rate of its ebbing and flowing. Nothing can be more irregular than both. Hence it is obvious that the common theory of the syphon, by which these intermitting fountains are explained, will not apply to this. Indeed, it is probable that it will not apply to most of the intermitting fountains, when properly examined. Mr. Gough gives another theory founded upon an observation made by Mr. Swainston, a manufacturer of Morocco leather at Kendal. It consisted in the irregularity with which water flowed through an inverted syphon. This was occasioned by the air mixed with the water collecting in the syphon, and thus diminishing its capacity. Mr. Gough conceives that in the case of intermitting fountains the water falls from a height into a reservoir, so as to occasion the formation of foam. From this reservoir there is a narrow winding passage, by which the fountain makes its way out of the earth. The foam enters into this passage along with the water, and the air collecting together partially choaks it up, and occasions a diminution in the flow: till at last, this air being expelled, the flow becomes as great as ever, and continues so till it is impeded by the evolution of new air. This explanation appears to me to account, in a satisfactory manner, for all the irregularities of the intermitting fountains.

12. *Description of a Eudiometer, and of other Apparatus employed in experiments on the Gases. By* William Henry, M.D. F.R.S. &c.] This eudiometer consists of a glass tube, close at one end, and graduated into 100 parts. The open end of the tube is ground into a very short open glass-tube, which is fixed, so as to be air-tight, into a small caoutchouc bottle. The gas to be examined is put into the glass tube, and the bottle is filled with the liquid destined to absorb the oxygen in the gas. The tube is then fixed into the bottle; and by inverting it the gas makes its way into the bottle, where it may be agitated with violence till the absorption is completed. It may be then measured with facility, and the quantity of absorption ascertained.

The other instruments described are, 1. A long narrow glass tube, 0·08 in diameter, which is accurately divided into ten equal parts, by drawing mercury by the mouth up to any determinate number of these divisions. By this means an equal bulk of mercury is always obtained, and any tube may be graduated with considerable rapidity. I have long employed a similar method of graduating tubes, and can attest its value and accuracy. I shall hereafter give an account of my method, as it is very easy, and enables us to graduate tubes with great neatness, as well as rapidity. 2. An apparatus for exposing gases to the long continued action of electricity. It consists of a small glass cylinder, terminating in a globular extremity. In this globe two holes are drilled, through which there pass two small glass tubes, which are hermetically sealed into the globe. Through these tubes pass small platinum wires, which are hermetically sealed by the melting of the tube round them. The ends of these tubes are ground away till the extremity of the wires are exposed. These tubes are so placed within the globe that their extremities are within the striking distance.

13. *A Memoir on the Uric Acid. By* William Henry, M.D. F.R.S. &c.] Dr. Henry gives a detailed and accurate history of the previous facts ascertained respecting uric acid. He then describes its properties, the salts which it forms, and its decomposition by heat. It may be obtained pure by dissolving urinary calculus, composed chiefly of it in potash ley, and mixing the solution with an excess of muriatic acid. A white powder falls, which, when washed in water, and digested in carbonate of ammonia is pure uric acid. This acid possesses the following properties:—

It has the form of white shining plates. Has no taste nor smell. It dissolves in about 1400 parts of boiling water. It reddens the infusion of litmus. When dissolved in nitric acid, and evaporated to dryness, it leaves a pink sediment. The dry acid is not acted on, nor dissolved by, the alkaline carbonates or subcarbonates. It decomposes soap when assisted by heat; as it does also the alkaline sulphurets and hydrosulphurets. No acid acts upon it except those which occasion its decomposition. It dissolves in hot solutions of potash and soda, and likewise in ammonia, but less readily. The urates may be formed either by mutually saturating the two constituents, or we may dissolve the acid in an excess of base, and then precipitate by carbonate of ammonia.

The urates are all tasteless, and resemble in appearance uric acid itself. They are not altered by exposure to the atmosphere. They are very sparingly soluble in water. They are decomposed by a red heat, which destroys the acid. The uric acid is precipitated from these salts by all the acids except the prussic and carbonic. They are decomposed by the muriates, nitrates, and acetates of barytes, strontian, lime, magnesia, and alumina. They are precipitated by all the metallic solutions except that of gold.

When uric acid is exposed to heat the products are carbureted hydrogen and carbonic acid, prussic acid, carbonate of ammonia, a sublimate consisting of ammonia combined with a peculiar acid.

14. *A Demonstration of Lawson's Geometrical Theorems, by the late* Rev. Charles Wildbore; *communicated by* Mr. Mabbott *to* Mr. Ewart, *and by him to the Society.*] This is a very curious paper, and well worth the attention of the admirers of the ancient method of geometry. But it is not susceptible of abridgment, nor could we make it intelligible without transcribing the whole paper, we must therefore satisfy ourselves with referring our mathematical readers to the essay itself; and we can promise them both amusement and instruction. Some of the figures are so complicated as to make it somewhat troublesome to consult them. This results from the method of the author, who has contrived to deduce the properties from each other in a very beautiful manner.

15. *Remarks on the Summer Birds of Passage, and on Migration in general. By* Mr. John Gough.] In this ingenious essay Mr. Gough shows that the opinion entertained by some that the swallow, and other migrating birds, pass the winter in a torpid state, is inconsistent with itself, and cannot be supported. The migrating birds feed on insects, and are compelled to change their quarters when their food disappears. He shows by direct evidence that the migrations of these birds, both to the north and the south, take place not at once, but slowly and gradually, according as the season advances, and gives facts that show that in some cases a week elapsed before the chimney-swallow moved a distance of six miles. He terminates his essay

with a curious table of the days of the month at which the different migrating birds usually make their appearance at Kendal, either on their way to the north or to the south.

ARTICLE XII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Concretion on the Elm.*

I lately received from William Elford Leach, Esq. a concretion of a brownish grey colour, and stony appearance, which he had found deposited on the bark of an elm tree, I believe in Hyde Park. All the specimens of it were mixed with more or less of the bark of the tree. I subjected it to chemical analysis, and found it composed of the following constituents:—

Old bark	28
Carbonate of potash	39
Carbonate of lime	32
Carbonate of magnesia	1
	100

Another specimen analysed contained no less than 48 per cent. of bark. The other constituents were nearly in the above proportion. Vauquelin long ago published the analysis of a concretion from the elm containing the very same constituents; but its appearance was different. He describes the substance which he examined as white, and says that he could distinguish crystals in it. The concretion collected by Mr. Leach was dark grey, and no crystals could be detected in it, by the most careful examination.

II. *Calculus from the Urethra of a Hog.*

I received some time ago from Mr. Colville, surgeon in Ayton, Berwickshire, a calculus extracted from the urethra of a hog, which deserves to be described. It is nearly spherical, weighs 44·2 grains, its specific gravity is 1·595, it is white, has a silky lustre, and is composed of a congeries of very small crystals, which, as far as can be judged by the eye, consist of flat four-sided prisms. The calculus is soft, so that the crystals are very easily separated from each other. This calculus is composed entirely of phosphate of lime; at least I have not been able to detect any other matter in it. When heated with potash, no smell of ammonia can be perceived, which I consider as excluding the presence of the triple phosphate. As the potash dissolves nothing, no uric acid can be present. The crystals dissolve without effervescence in muriatic acid, and are precipitated in the state of a white powder by pure ammonia. These characters I consider as excluding every thing but phosphate of lime.

III. *Hydrosulphurets.*

Thenard has lately published some observations on the hydrosulphurets, which deserve the attention of chemists. I shall for that reason give a short account of them in this place :—

1. When a saturated hydrosulphuret is heated along with sulphur, a portion of the sulphur is dissolved, and a quantity of sulphureted hydrogen gas escapes. If there be an excess of alkali, the sulphur is dissolved as usual; but little or no sulphureted hydrogen escapes. Hence we see that the hydrogureted sulphurets contain less sulphureted hydrogen than the hydrosulphurets.

2. When the saturated hydrosulphurets are raised to the boiling point, a portion of sulphureted hydrogen always makes its escape. By this method the hydrosulphuret of magnesia may be decomposed altogether, and hydrosulphuret of lime nearly so. Hydrosulphuret of potash and soda become very alkaline.

3. Hydrosulphuret of ammonia may be obtained in crystals by surrounding with ice a flagon containing a mixture of sulphureted hydrogen and ammoniacal gases. It crystallizes in needles, and is white; but becomes speedily yellow when exposed to the air. It is very volatile, rising spontaneously to the top of the phial in which it is kept. By this means it may be separated from the hydrogureted sulphuret of ammonia.

4. When ammoniacal gas and sulphur are passed together through a red-hot porcelain tube, hydrogen gas and azotic gas are disengaged, and a great quantity of hydrogureted sulphuret of ammonia crystallized. When this substance is put into a phial, pure crystals of hydrosulphuret of ammonia gradually sublime from it.

5. The fuming liquor of Boyle smokes in oxygen gas or common air; but not in azotic gas or hydrogen gas. Hence the smoking must be owing to the presence of oxygen. Thenard supposes that it acts by converting the liquor into hydrogureted sulphuret, or into sulphite. (See *Ann. de Chim.* lxxxiii. 132.)

IV. *Antimonial Acids.*

No metal has been subjected to a more persevering examination than antimony. But its chemical properties are so difficult of investigation, that the most accurate and ingenious chemists have contradicted one another in their most recent experiments. Thenard found six oxides of antimony, Proust reduced them to two, and Berzelius makes them four. Theory is certainly in favour of the last opinion. The two oxides of antimony containing most oxygen possess, according to Berzelius, the properties of acids. He calls them *antimonious* and *antimonic acids*. The antimonious acid is white. The *calx antimonii elota* of the old pharmacopœias is this acid combined with potash, and containing an excess of acid. When boiled in water a neutral antimonite of potash is obtained, from which other antimonites may be formed. The antimonite of barytes crystallizes in white needles, and is not altered by exposure to the air.

The antimonic acid has a straw yellow colour. It is formed by heating antimony in nitro-muriatic acid. Berzelius has examined several of the saline combinations which it forms with the different bases.

The two acids of antimony have the property of combining together, and of uniting likewise with the other oxides of that metal. This makes it so difficult to examine the oxides of antimony with accuracy. (See Berzelius, *Lärbok i Kemien*, ii. 159.)

V. *Acid of Tin.*

The peroxide of tin has been repeatedly considered as an acid by chemists. Bergman and Guyton Morveau have each published dissertations on the subject. Berzelius has lately examined the matter anew; and though he admits that it possesses some characters in common with acids, yet he thinks that they are not sufficient to entitle it to the name.

VI. *Acid of Tellurium.*

The oxide of tellurium, according to the observations of Berzelius, possesses at once the characters of an acid and a base. Hence it is capable of combining with bases and forming salts, while at the same time it unites with acids, and forms another kind of saline compound. On that account we may either call it *telluric acid* or *oxide of tellurium*.

VII. *Sulphite of Copper.*

It is well known to chemists that there are two oxides of copper, the red and the black; the first composed of 1 atom of metal and 1 atom of oxygen, the second of 1 atom of metal and 2 atoms of oxygen. The red oxide refuses to combine with sulphuric acid. When the two substances are brought in contact, the red oxide of copper divides itself into two equal portions, one of which gives all its oxygen to the other; so that one half of the oxide is reduced to the metallic state, and one half converted into black oxide. This last half unites with the sulphuric acid, and forms common sulphate of copper. The black oxide refuses to combine with sulphurous acid. When the two substances come in contact, the black oxide gives out half its oxygen, and by this means is changed into red oxide, while a portion of the sulphurous acid is changed into sulphuric acid.

Sulphite of copper is of a red colour, and crystallizes. It may be obtained by mixing sulphite of potash and sulphate of copper together, or by passing a current of sulphurous acid gas through water, in which black oxide of copper is suspended. It is decomposed by heat, and by boiling in water. According to the experiments of Chevreul, it is composed of

Red oxide of copper	63·84
Sulphurous acid	36·16
	100

But this analysis cannot be accurate. It gives us 100 acid + 176·549 oxide. Now the weight of an integrant particle of sulphurous acid is 4, and of red oxide of copper 9. Hence if the two substances unite particle to particle, the compound must consist of 100 acid + 225 oxide. We learn from the experiments of Berzelius that sulphuric and sulphurous acids unite with the same weight of base. Now according to him 100 sulphuric acid unite with 183 of red oxide of copper. (*Ann. de Chim.* lxxvii. 83.) Hence 80 sulphurous acid would unite with 183 red oxide. This gives us 100 acid + 228·75 red oxide, which agrees very nearly with an atom of acid and an atom of oxide. Hence I conceive it to be indisputably much nearer the truth than Chevreul's analysis. The reader will find Chevreul's experiments in the Ann. de Chim. vol. lxxxiii. p. 181.

Chevreul obtained likewise a triple salt, composed of sulphurous acid, potash, and red oxide of copper. He procured it by mixing a cold solution of sulphite of potash with nitrate of copper. The triple salt precipitated of a yellow colour. According to his analysis, it is composed of

Red oxide	0·9360
Potash	0·1556
Acid	0·6270
	1·7186

This analysis would stand in need of some animadversions; but they would not be understood till we have come, in our table of the weight of integrant particles of chemical compounds, to the triple salts. We shall therefore defer the subject at present.

VIII. *Freezing of Alcohol.*

We have received a letter on this subject from a Gentleman, who subscribes himself Philo-Chemicus Oxoniensis, who conceives that the method employed by Mr. Hutton, of Edinburgh, to freeze alcohol, may have been similar to that already practised by Mr. Walker, of Oxford; namely, employing bodies already frozen as instruments of congealing others. "This," he observes, "has often been done in the congelation of mercury, when ice, already reduced to a certain temperature, is mixed with muriate of lime; nor do I see any limit to the intensity of cold which might be produced in this manner, except the point at which the attraction of aggregation equals in activity (in consequence of the subtraction of so much caloric) the strength of affinity which the substance, whatever it may be, has for any other body. Thus sulphate of soda and muriatic acid sink the temperature of water from 50° to 0; this, again, when mixed with muriate of lime, would lower the temperature of any surrounding body from 0 to — 66°; and ice, which had been cooled down to this point, might, by the addition of sulphuric acid in a certain state of dilution, depress the thermometer to — 99°. All these facts are well known; and if this should happen to be Mr. Hutton's method, it deserves to be considered in the light rather of a more extended application of what had been before ascertained than the discovery of any new fact. The plan I have mentioned, although it might not enable us to congeal any fresh substances, would at least, I should conceive, establish at what point the attraction of aggregation equals that of affinity. The most convenient apparatus, perhaps, would be a series of circular vessels, the one contained within the other, in the innermost of which should be placed that which was to be last operated upon."

IX. *Composition of Azote.*

In consequence of a notification in our last Number respecting the composition of azote by Professor Berzelius, who, by calculation, has concluded it to be composed of 44·6 unknown inflammable basis and 55·4 oxygen, I have been informed that Mr. Miers, a young chemist of London, has proved, by a number of experiments, the compound nature of this gas, which has hitherto resisted all attempts at decomposition. He finds, however, that the "inflammable basis" is not "unknown," but that it is hydrogen; and the proportions in which it is combined with oxygen do not, from his experiments, greatly differ from those calculated by Professor Berzelius. The experiments involving this important discovery were made about 15 months since; and although their public annunciation has been delayed with a view to more delicate and refined results, professional engagements have since prevented their being completed. The further prosecution of these labours will be conveyed through the medium of this Journal, at a period not far distant.

Article XIII.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday, May the 27th, a paper by Mr. Cater was read, comparing the Cassegrenian and Gregorian telescopes. These telescopes have been hitherto considered as quite similar. Mr. Cater was led to compare them in consequence of telescopes of both kinds being constructed by a self-taught artist at Ipswich, who has acquired the art of constructing both in remarkable perfection. The result of the comparison was, that the Cassegrenian telescope gave a much clearer and better defined image of the object than the Gregorian. Mr. Cater endeavours to account for this difference, by supposing that in the Gregorian telescope the particles of light interfere, and impede one another; while this does not happen in the Cassegrenian.

On Thursday, June the 3d, part of a paper by Mr. Brande was read, containing additional facts and observations on the use of magnesia in cases of urinary calculus. The paper was divided into two sections. In the first, Mr. Brande related several cases in which the deposition of uric acid in urine and the accompanying symptoms were removed by the use of magnesia. Among others, a Gentleman of fifty-five was afflicted with pain in the kidney; a calculus at last passed into the bladder, and was voided by the urethra: it consisted of uric acid. His urine deposited a considerable quantity of red sand (uric acid). He tried alkalies; but they disagreed with his stomach. He was induced, in order to alleviate the symptoms of indigestion, to take a tea-spoonful of magnesia daily. His symptoms were gradually removed, and the urine ceased to deposite red sand. Several similar cases were given, in all of which the deposite of uric acid was put an end to by the use of magnesia. But in one case, after a certain time, the symptoms were aggravated, and a white sediment was deposited. This turned out, on examination, to be a mixture of phosphate of lime and phosphate of magnesia-and-ammonia. The object of the second section of the paper was to give an account of the treatment in such cases. Acids were administered, and different kinds were tried. Carbonic acid in the form of soda water, citric acid, vinegar, cyder, oranges, lemonade, and muriatic acid, were tried in succession. The muriatic acid seemed to have answered worst. The other acids, in several cases related, removed the symptoms without inducing a deposition of uric acid.

On Thursday, the 17th June, Mr. Brande's paper was concluded. Another case was related, in which the deposition of the phosphates in the urine was put a stop to by the use of carbonic acid, produced by mixing lemon juice and carbonate of potash, and drinking the mixture while in a state of effervescence. From these cases Mr. Brande concludes,

1. That when the alkalies, from any circumstance, cannot be used to put a stop to the deposition of uric acid in the urine, then magnesia may be employed with advantage.

2. That the deposition of the phosphates may be put a stop to by the use of mineral acids.

3. That the vegetable acids produce a similar effect, and that they may be employed in much greater quantity, without injuring the digestive organs.

On the same evening a paper was read by Mr. Axlay, of Bristol, on the phenomena of electricity. He began by stating what he considered as his peculiar theory of electricity; but which does not appear to differ from that of Cavendish, unless the fourth proposition be considered as peculiar. The theory consisted of the following propositions.—

1. A fluid exists called the electric fluid.

2. It is attracted by all matter, with a force inversely as some power of the distance.

3. Its particles repel each other. Hence it is elastic and compressible.

4. Electrics have a stronger affinity for it than non-electrics.

From this theory he explains the different phenomena of electricity; very nearly, as far as I could judge, without seeing the figures to which a reference was continually made, in the way already employed by Mr. Cavendish, and other electricians; with this exception, that he does not suppose that matter is repellent of matter, as follows from the theory of Æpinus.

On Thursday, June the 24th, a paper by Sir Everard Home, Bart. was read, containing additional observations on the squalus maximus, or great shark. Two of these fishes were caught at Brighton last December, and one of them was brought to London. It was particularly examined by Sir Everard and Mr. Clift, and this paper stated the result of the new observations. The figure given by Sir Everard along with his former paper is correct, except that a fin between the anus and tail is wanting. The liver is very tender, and consists of six lobes. The gall duct is dilated at the extremity which enters the intestines, the object of which seems to be to prevent the bile from returning into the liver. There is no gall bladder. The heart is very powerfully muscular, and there is a particular muscle connected with the valves, which Sir Everard conceives intended to impel the blood more powerfully through the gills when the animal is at a great depth under water; for the pressure of the water will in that case impede the circulation in the gills. It occurred to Sir Everard that this impeded circulation might be compensated by water at great depths containing more oxygen gas in solution than near the surface; but water being taken up from the bottom of a deep well, and examined, was found to contain no more than water at the surface. It may be remarked here, that this trial was scarcely of such a nature as to be decisive. Well water, however deep, is nothing else than rain water which has made its way through the earth to the bottom of the well; and all of it having been equally exposed to the air when falling in the state of rain ought to contain the same portion of air. The water examined ought to have been taken from the bottom of the sea; but it is not likely that any perceptible difference would have been found. Indeed, the experiment has been already made by Biot, who examined sea water taken up at the depth of 437 fathoms, and found the proportion of oxygen gas in it the same as at the surface. (*Mem. d'Arcueil.* i. 273.) But the swimming bladder being filled with oxygen gas in those fishes that live at great depths, this in all probability is intended to answer some such purpose.

Sir Everard compared the heart of the squalus with that of several other animals. The squalus has no cerebrum, but only cerebellum. The cavity in which the semicircular canals of the ear is placed is uncommonly large. The lens of the eye is globular, and half sunk in the vitreous humour, which is very firm, and lodged, as usual, in separate cells. The retina is very thin. The cornea consists of three coats.

LINNÆAN SOCIETY.

The Society met upon Tuesday the 1st, and Tuesday the 15th of June. Both meetings were occupied in hearing read a paper by Col. Hardwicke, on the bats in the British dominions in India. He described and exhibited figures of 11 species, most of which inhabit trees, and live on fruits. The most remarkable of these is a very large species, having the aspect of a wolf, and of such a size that, from tip to tip, the wings extend 3 feet 8½ inches. This species the Colonel considers either as the vampyre of Linnæus, or as nearly allied to it. It lives on fruits, and is considered by the Indians as quite harmless. The Colonel found that it would eat raw flesh when hungry; but that it preferred fruits. The stories of its sucking the blood of living animals he considers as quite unfounded.

The Society adjourned till the 2d of November.

GEOLOGICAL SOCIETY.

This society was established some years ago, for the express purpose of accurately investigating the geognostic structure of Great Britain. The activity of the members, and the great progress which they have already made, is really surprising, and entitles them to a very high place in the catalogue of useful institutions. At the last general election it consisted of 115 ordinary, and 108 honorary members; and, as many elections have been made since that period, the number of ordinary members is at present considerably increased. I shall give a short account of the papers which have been read at their different meetings during the present session, which began on November the 6th, 1812.

On Friday, Nov. 6, 1812, a letter was read from Ed. L. Irton, Esq. on the sand-tubes found at Drigg, in Cumberland. These tubes have been found only in a single hill of drift sand on the sea-shore, about five acres in extent. They were discovered by the drifting of the sand. They are placed nearly perpendicularly, at unequal distances. One was traced 15 feet deep, but how far they go is unknown. When first dug out they are flexible, but soon become quite rigid. Internally they have a glaze, which is perfectly vitreous.

An account, by Dr. Macculloch, of a remarkable vein in a mill-stone of blue limestone. This mill-stone was shipped from Limerick, and is at present at the Royal Powder Mills at Waltham Abbey. The vein is a whitish compact carbonate of lime. This vein, in its present state, consists of angular fragments having a certain parallelism, and leaving no doubt that they once constituted a regular contiguous vein. By what mechanism have they been brought into their present state?

On Friday, Nov. 20, some observations on a bed of greenstone near Walsall, in Staffordshire, by Arthur Aikin, Esq. were read. This greenstone occurs in the independent coal formation, and Mr. Aikin considers it as a vein branching off a dyke of greenstone that occurs near it. It consists of felspar, mixed with calcareous spar, and minute grains of augite and hornblende. The substances below this greenstone are considerably altered, when compared with their appearances in other parts of the field. These are *sandstone*, which is indurated; *shale*, which is deprived of its bitumen; and *coal*, which is also deprived of its bitumen, and more friable than in other places. These changes, as they do not occur in other parts of the field, Mr. Aikin considers as connected with the presence of the greenstone.

On Friday, Dec. 4, part of a paper by William Philips, Esq. on the veins of Cornwall was read. Most of the metalliferous veins of Cornwall run east and west. They have frequently been followed for two miles; but in no instance traced to a termination, either in length or depth, being always relinquished when no longer worth working. The deepest mine in Cornwall, Dolcoath, is 228 fathoms below the surface. They vary in thickness, from one foot to three; though there are instances of veins 24 or even 30 feet wide. The veins are distinguished by names, according to the nature of the veinstones. The following are the principal:—1. *Gossany*, when the veinstone is clay, mixed with silica, and oxide of iron. Its colour varies from light yellow to deep brown. This is the most common veinstone, and is considered as promising both for copper and tin. 2. *Sparry*, when quartz predominates. It is rather unpromising. 3. *Mundicky*, when iron pyrites abounds. It is considered as rather promising. 4. *Peachy*, when the veinstone is chlorite. It is more promising for tin than for copper. 5. *Flookany*, when one or both of its sides is lined with bluish white clay. 6. *Caply*, when the veinstone is a hard substance of a greenish or brownish colour, chiefly a mixture of chlorite and quartz. Tin is found in it; but seldom copper. 7. *Pryany*, when the one is found in detached lumps. 8. When a vein abounds in blende it is called a *black jack lode*; when it contains granite it is called a *growan lode*.

On Friday, Dec. 18, Mr. Philips's paper on the veins of Cornwall was continued. The contents of a vein are divided into those which are valuable, and those which are not so. The latter are called *deads*, and are left as much as possible in the vein. Sometimes large wedge-shaped fragments of rock occur in veins called by the miners *horses*, partially cutting off the contents of the vein, though seldom entirely obstructing it. Veins of copper ore are liable to total obstructions without any obvious cause. In proportion as the rock becomes harder, the vein always becomes narrower.

Copper mines were formerly considered as peculiar to slate; but of late they have been found to pass freely from slate to granite, and back again, without deterioration.

The metalliferous, or east and west veins, are crossed by others, the direction of which is nearly north and south. These are called cross courses, and rarely produce any metallic substance. There is another species of vein, called a *contre* or *counter*, the direction of which is for the most part north-east and south-west. They are usually metalliferous, and often very rich.

On Jan. 1, 1813, Mr. Philips's paper on the veins of Cornwall was concluded. The remainder of this very valuable paper consisted of accounts of particular Cornish veins, not very susceptible of abridgment.

On Jan. 15, a paper by Mr. Conybeare was read, on the origin of a remarkable class of organic impressions occurring in nodules of flint. It consists of small round compressed bodies, not exceeding the eighth of an inch in their longest diameter, and connected by processes of the fineness of a hair. Mr. Conybeare shows clearly that they occur between the bony plates of a bivalve shell, the ostreo pennite of Welsh, and likewise in another shell, probably belonging to the genus ostræa. He conceives them to be casts of the cells of some minute parasitical insect inhabiting the shells in question.

On the 19th of Feb. a paper by John Taylor, Esq. was read, on the economy of the mines of Cornwall and Devon. The stannary laws are confined to the tin mines; the copper mines are left open to the agreement that may be made between the proprietors of the soil and the mining company. In general, the lord of the soil grants a lease for 21 years, subject to certain regulations. The adventurers divide the whole concern into sixty-four shares, which are divided among themselves and their friends. The mines are wrought by the piece, and the working disposed of by a kind of auction. The smelting companies for copper have seldom any share in the mines. The copper is all carried to Swansea, where it is smelted.

On the 5th of March, two letters from Mr. Webster, draughtsman, and Keeper of the Museum to the Society, were read, giving an account of a calcareous strata of later formation than chalk, and distinguished by containing fresh water shells, discovered by him on the north side of the Isle of Wight. He thinks there are two fresh water formations, with a marine formation between them. The lowest consists of beds of sand and marl, with numerous fragments of the limnea of Lamark, and two species of planorbis. The marine formation lying over it is blue clay, with venuses, oysters, and various turbinated shells. The upper fresh water formation consists of a calcareous rock, inclosing numerous and very fine specimens of the limnea and planorbis.

A paper by Dr. Macculloch on the granite tors of Devonshire and Cornwall was read. The granite in these counties is naturally split into cubic fragments by perpendicular and horizontal rents. These, by exposure to the weather, occasion the rock to split into fragments, the edges of which are gradually rounded off. Dr. Macculloch supposes that crystallization began in the centre of each of these cubic blocks, and extended on every side till they met, but could not penetrate. By this hypothesis he accounts for the rents, and for the greater hardness in the centre of the blocks than at the circumference.

On the 19th of March a paper was read on the rocks at Clovelly, in Devonshire, by the Rev. J. J. Conybeare. These rocks lie on the sea-shore on the north coast of Devonshire. They are precipitous, and about 140 feet high. They consist of alternate beds of grey wacke and grey wacke slate, the first of which is called *dimstone*, the second *shillat*. This rock only contains organic remains where it alternates with transition limestone. Killas is considered by the author as *clay slate*. It is traversed by frequent veins of a porphyritic rock, which does not enter into the grey wacke, contains sometimes topaz, and frequently garnet. Its veins are often filled with chlorite, mica, and crystallized felspar, and also contain tinstone, grey cobalt ore, &c.

A paper on the Island of Staffa, by Dr. Macculloch, was also read. This island is about two miles in circumference. It forms a kind of table land, bounded on all sides by steep cliffs, from 60 to 70 feet above high water mark. The highest elevation of the island is 120 feet. The whole island is a mass of basalt.* The basalt occurs in two states, columnar and amorphous; the latter is usually amygdaloidal, and contains zeolites. On the south-western side of the island there appear to be three distinct strata of basalt; the lowest is amorphous; the second is from 30 to 50 feet thick, and consists of those columns which constitute the conspicuous feature of Staffa. The uppermost stratum consists of small columns. The surface of the island is covered in many places with rounded fragments of granite, gneiss, mica slate, quartz, and red sandstone, together with a few rolled pieces of basalt.

(*To be continued.*)

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the French Institute for 1812,

(*Continued from* Vol. I. p. 396.)

BOTANY AND VEGETABLE PHYSIOLOGY.

Most physiologists have long admitted in plants an ascending sap, which mounts from the roots to the branches, and contributes to the increase of the branches in length; and a descending sap, which goes from the leaves to the roots, and to which some persons ascribe the chief part of the growth of the wood, and of course the increase of size in the trunk.

M. Feburier, a cultivator at Versailles, has endeavoured to collect these two saps separately. For this purpose he bored a deep hole in the trunk of a tree, and fixed a bladder upon the inferior surface, so as to prevent any liquid coming from the lower part of the tree from making its escape into this hole. He made another hole in the tree, and placed a bladder in the same way against the upper surface. He considered the sap collected in the lower bladder as the ascending sap, and that collected in the upper bladder as the descending sap. He gives many observations on the relative proportions of each in different circumstances. Wishing, in the next place, to determine the route which each of these saps takes in the inside of the tree, he plunged alternately by the two ends branches of trees in coloured infusions. In both cases these infusions appeared to him to follow the woody fibres surrounding the pith. This induces him to ascribe the same route to both saps, in which respect he coincides with the result of other experiments made by Mustel.

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* From specimens of Staffa shown me by Lieut. Col. Fullerton, which he brought himself from the island, I am inclined to consider the rock not as basalt, but as porphyry slate. [*Note of the Editor.*]

M. Feburier thinks, likewise, that the ascending sap contributes principally to the growth of the branches, the descending to that of the roots. But he thinks that the *cambium*, or that humour which transudes horizontally from the trunk, and which is looked upon as the matter which occasions the increase of the tree in thickness, proceeds, like the peculiar juices, from the mixture of the two saps. The presence of the leaves, necessary for the production of the descending sap, is of consequence also for the increase of the plant in thickness. But the buds, which M. du Petit Thouars conceives to act so important a part in that operation, have nothing to do with it, in the opinion of M. Feburier; for it takes place, says he, as long as the leaves exist, and it ceases as soon as they are removed, whether the buds be left or not.

As far as concerns the flowers and the fruits, M. Feburier assures us that he has observed the ascending sap, when it predominated, tending to determine the production of simple flowers, and the complete developement of the germs; that the descending sap, on the contrary, when too abundant, occasions the multiplication of flowers and petals, and the growth of the pericarp, and by consequence of the fleshy part of the fruit; principles from which it would be easy to deduce many useful practices in gardening, and which explain various practices already pointed out by experience.

According to M. Feburier, the alburnum deprived of its bark, but kept from the contact of the air, is capable of reproducing, by means of the cambium, the bark and epidermis necessary to cover it, as the bark produces constantly, even when partly separated from the trunk, liber and alburnum. In this point he has for antagonist our colleague M. Palisot de Beauvois, who has likewise employed himself in the investigation of these difficult questions, respecting the direction of the sap, and the formation of wood. According to this botanist, this escape of a glossy humour, which some physiologists suppose to proceed from the old alburnum, and to contribute to the formation of the liber, is not founded on convincing experiments. On the contrary, when a portion of bark is removed from a tree, and the wound is well rubbed, so as neither to leave liber nor cambium, neither the alburnum nor the wood produce any thing; but the edges of the bark gradually extend themselves, cover the naked wood, and produce liber and alburnum incontestibly proceeding from that bark. M. de Beauvois announces that he will soon elucidate this proposition at full length, which hitherto he has only noticed incidentally in his Dissertation on the Pith of Vegetables.

The opinion of physiologists has been hitherto very much divided about the functions of the pith of vegetables. According to some, that organ is necessary to the life of plants during the whole of their existence: according to others, it is only useful during the first years of their life, or all the time that it is green and succulent, and may be easily confounded with the cellular texture. M. de Beauvois has made on this subject observations which tend to prove that the pith performs functions during the whole life of the plant, if not absolutely necessary to its existence, at least very important for its progress, and the growth of its branches, leaves, and especially of the organs necessary for its reproduction.

He has remarked that the circular layer, or fibres which immediately surround the pith, has always a form corresponding to the arrangement and disposition of the branches, twigs, and leaves: that in plants with verticillated twigs and leaves, for example, the horizontal section of this case of the pith shows as many angles as there are twigs at each stage, and at each verticilla.

In like manner, the medullary case of the laurel rose presents an equilateral triangle, if the branch below the verticilla has three twigs and three leaves; but if we cut it under the lower verticilla, where one of the twigs and leaves is usually wanting, it has only two angles, and the vestige of a third. This law is constant, even in herbaceous plants,

M. de Beauvois has begun similar observations on plants, with opposite, alternate, dichotomous, spiral, and pinnated leaves. He expects to find in them the same relation between the medullary case, and the disposition of the branches, twigs, and leaves. For example, opposite leaves seem to require a round medullary case, becoming oval, and having its extremities more and more pointed the nearer it approaches to the insertion of the branches and the leaves. When the leaves are alternate, the circle is less perfect. The extremities become equally narrow, but alternately, and each on the side where the branch is to appear. When the leaves are spiral, the number of angles of the medullary case is equal to that of the leaves of which the spirals are composed. Thus the medullary case of the lime has four angles; that of the oak, the chesnut, the bramble, the pear-tree, almost all the fruit trees, &c. has five angles, more or less regular, because the spirals succeed constantly in fives.

Grew and Bonnet alone seem to have been on the way of these observations. The first had observed very various forms in the medullary case, especially in that of the conical roots of culinary plants; but he did not observe the relation between these forms, and the dispositions of the branches and leaves. The second carefully examined vegetables with opposite, verticillated, alternate, and spiral leaves; but did not observe the connection of these dispositions with the form of the medullary case.

M. de Mirbel has continued his researches into the structure of the organs of fructification in vegetables, in which he has been seconded with a zeal and intelligence that he takes a pleasure in acknowledging, by M. Schubert, whom the Grand Duchy of Warsovia sent into France to complete his knowledge of botany, which he was afterwards to teach in Poland. These two botanists have examined all the genera of trees with needle-shaped leaves, or the coniferous, one of the most important to be known, on account of the singularity of its organization, of the greatness of the species which it includes, and of the utility of its products. Every body is able to distinguish, at the first glance of the eye, the cedar, the meleza, the spruce fir, the Scotch fir, the thuya, the cypress, the yew, the juniper; but though the botanists have studied with particular attention the organs of reproduction of these vegetables, they are not agreed about the characters of the female flower; or to speak more correctly, the greater number are of opinion that the stigmata of the spruce fir, the Scotch fir, the cedar, and the meleza, have not yet been observed. In this respect, therefore, these trees are in reality cryptogamous. MM. Mirbel and Schubert go farther: they affirm that the female flower of the yew, the juniper, the thuya, the cypress, &c. is not better known; and that all the genera of coniferous plants have a common character, which has hitherto misled observers, and which consists in the existence of a cup, not similar to that in the flower of the oak, but more deep, concealing entirely the ovarium, and contracted like the neck of a bottle at its orifice. The female flower enclosed in this case has escaped observation. In the thuya, yew, juniper, cypress, &c. the cup is strait; and by an error, occasioned by the extreme smallness of the organs, the orifice of this cup has been always mistaken for the stigma. In the cedar, meleza, spruce fir, and Scotch fir, the cup is reversed, and its orifice is very difficult to perceive. It is only within these few years that it has been observed in England by Mr. Salisbury, and in France by MM. Poiteau, Mirbel, and Schubert. These botanists have not hesitated to consider it as the stigma; and this was natural, because it had been agreed upon that the stigma of the yew, the thuya, the cypress, &c. was placed at the orifice of the cup: but farther researches have undeceived MM. Mirbel and Schubert. By a delicate dissection, they have ascertained that what is generally taken for the female flower in coniferous plants is only the cup, the form of which resembles that of a pistil, and that it contains in its cavity the true flower, which is provided with a membranous calix, adhering to the ovarium, and with a stigma, sessil in all the genera except the *ephedra.*

It must be obvious that this structure, so different from what had hitherto been imagined, will occasion great changes in the explanation of the characters, of the family, and of the genera.

According to Mirbel, the female flower of the plants of the family of *cycas* has an organization analogous to that of coniferous plants. This supports the opinion of M. Richard, who places these two families together among dicotyledonous plants; but M. Mirbel is of opinion, that as long as the characters of vegetation serve for the base of the two great divisions of plants with visible flowers, the *cycadeæ* cannot be separated from the palms.

The organization of the male flower of mosses has likewise engaged the attention of Mirbel and Schubert. After Hedwig, it would have been difficult to discover any thing new on this subject; but the bursting of the anthers, and the emission of the pollen, were phenomena doubted of by several botanists. Our two botanists declare that they have seen the most unequivocal proofs of the existence of these phenomena. The organs which Hedwig calls male in the *Polytrichum commune*, placed upon water, split at the summit, and threw out an oleaginous liquid, which extended itself like a thin cloud on the surface of the water. Mirbel and Schubert then subjected to comparative observation the pollen of a great number of *phenerogamous* plants, and ascertained that they exhibited the same phenomena as the male organs of mosses. This induces them to think that the parts called anthers by Hedwig may be nothing else than simple grains of pollen of a particular shape.

M. Mirbel has continued his researches on germination. He has observed, contrary to the generally received opinion, that the radicle does not always make its appearance first. In many cyperaceous plants it is always the plumula which appears first. The same botanist has republished, with important modifications and additions, his opinions respecting the organization of stems, respecting their growth, and respecting the structure, both internal and external, of the organs of fecundation of plants.

M. Henri de Cassini, son of one of our associates, a name so celebrated in astronomy, has presented to the Class a memoir, which augurs success in a different science. He has examined with peculiar care the style and stigma in the whole family of plants known by the name of *compound*, *syngenesious*, or *synantherous*; and these small organs have exhibited a number of curious differences, which have induced him to propose a division of these plants, founded solely on the modifications of these two parts of the pistil. We regret that we cannot follow this skilful observer in the details into which he has gone, and which he has described and drawn with remarkable neatness. It cannot be doubted but they will one day be of great service in perfecting the classification of this family, so numerous and so natural; the subdivision of which, in consequence, ought to be more difficult than of any other.

There are few families of vegetables so directly useful to man as the grasses, which comprehend wheat, rye, rice, mais, sorgho, sugar-cane, barley, oats, millet, &c. To name these plants is enough to show the importance of a work which would enable us to distinguish them with certainty. The characters hitherto employed are generally regarded as insufficient. At each step the observer finds himself stopped. It is difficult, indeed, often impossible, to find the true genus of the plant which he examines. Frequently the characters adopted only apply to a few species, and do not occur in the rest of the genus. M. Palisot de Beauvois has undertaken a general examination of this family, which he has published under the name of *Essai d'Agrostographie*. He has endeavoured to put an end to all this confusion, and to give to each genus constant characters, easy to perceive, so that the observer can never be at a loss.

For this purpose he has been obliged to adopt new bases, which he has already announced in his *Flora of Oware and Benin*. These depend principally on the separation or union of the sexes, on the composition of the flower, and on the number of its envelopes. Twenty-five plates, in which all these characters are represented, facilitate the study of these plants, which interest all the orders of society, even those persons who do not make botany a peculiar study.

M. de Beauvois continues his *Flore d'Oware et de Benin*, the thirteenth number of which is published, and his *History of Insects* collected in Africa and America, the eighth number of which has appeared.

M. de la Billardiere has finished his collection of the rare plants of Syria and Libanus, by the fourth and fifth parts. The same naturalist has communicated to the Class peculiar and interesting observations in natural history, which he made during his voyage in the Levant, the publication of which has been interrupted by the longer and more dangerous voyage which he made in the Entrecasteaux, an account of which was given to the public several years ago.

M. Gouan, correspondent of the Class at Montpellier, has published a description of the generic characters of the *ginko biloba*, a singular tree of Japan, which has been long known in Europe, but which, never having blossomed, could not be properly classified in the system.

There is a family of plants much less important than the grasses in point of utility, but much more singular in their characters, and which can only be seen vegetating on the sea-shore, namely, the *fuci*, and marine plants analogous to them. M. Lamouroux, professor of natural history at Caen, placed favourably in a city so near the coast, has made these plants a particular object of study. He gives them the common name of *thalassio-phytes*, and divides them into various tribes, the characters of which he has been obliged to take in all parts of the plant, because he could not find sufficient characters in the organs of fructification, which usually serve as a basis to these divisions; but which are too little known in most of the fuci, to be able to confine oneself to them. This is another of those laborious and useful undertakings which we have to regret that we cannot analyse in a summary so short as ours. Let it suffice to join our voice to that of the Commissioners of the Class, to request the speedy publication of it.

(To be continued.)

ARTICLE XIV.

List of Patents.

Samuel Evans, Denbigh, farmer; for certain improvements in the working, or giving motion to, mill-work and machinery, applicable to raising or drawing water from mines, and other useful purposes. Dated May 1, 1813.

Thomas Walker, Norwich, machine-maker; for various improvements in the construction of a horizontal windmill, that may be applied to all sorts of machinery that is to work by wind. Dated May 5, 1813.

William Reid, of Foot Dee, Aberdeen; for an instrument for calculating, without reference to tables of any kind (except meridional tables), the various problems in navigation, practical mathematics, and trigonometry, heights and distances, and altogether embracing every science depending upon angles. Dated May 5, 1813.

Charles Broderip, London; for improvements in vessels to be used for heating fluids and other substances. Dated May 5, 1813.

John Erat, harp-maker, London; for improvements in the construction of a pedal harp. Dated May 8, 1813.

Thomas Daking, Essex, tanner; for a method of heating liquors for the manufacture of leather, and other manufactures. Dated May 8, 1813.

John Fisher, Millend, Buckingham, and Layton Cooke, London, land agent; for improved gaiters, and modes of fastening the same. Dated May 11, 1813.

William Bullock, London, locksmith and founder, and James Boaz, Glasgow, engineer; for certain improved machines, instruments, or contrivances, applicable to doors and window-shutters, for preventing them from being broken open, and such doors from being violently forced in by the wind or otherwise, part of which may be applied to other useful purposes. Dated May 15, 1813.

Edward Cowper, of St. Mary, Newington Butts, Surrey, ironmonger; for certain improvements in the machines commonly used for cutting the edges of paper and books. Dated May 20, 1813.

William Jenkins, of Birmingham, brass-founder; for an improvement in the method of manufacturing socket castors, used with, or affixed to, cabinet and other furniture and things. Dated May 22, 1813.

John Thackray, London, cabinet-maker; for a method of enclosing a seat in a portable stool-stick, which seat may be applied to other useful purposes. Dated May 22, 1813.

Thomas Willcox, Bristol, mason; for a machine for curing smoky chimneys, consisting of a hollow cap, with a funnel and contrivance for voiding the smoke, to be fixed on the top of a chimney-stack with two or more courses of brick-work, and having for its object to prevent the smoke arising from the fire in the grate being driven back into the room, as well by excluding all winds from the orifice of the chimney, as by promoting the draft of the chimney, by means of a continued accumulation of rarified air in the cavity of the cap. Dated May 22, 1813.

William Brunton, Butterley Iron Works, Derby, engineer; for the construction and erection of engines, and other mechanical operations. Dated May 22, 1813.

William Stocker, gunsmith, of Martock, Somerset; for a cock made of metal and wood, for drawing liquor from casks, which produces a stop superior to that which is effected by common cocks, and prevents the liquor from coming in contact with the metals, except when the liquor is in the act of being drawn and is running from the cask. Dated May 25, 1813.

Article XV.

Scientific Books in hand, or in the Press.

Dr. Marshall Hall, Royal Infirmary, Edinburgh, is preparing a practical work on the physiognomy and attitude of patients, and on the symptoms and diognosis of diseases. The whole of the work is intended to bear on diognosis as its principal and ultimate object.

Dr. Robert Wall, of Glasgow, has a work in the press on the History, Nature, and Treatment of Chincough, illustrated by a variety of cases and dissections; to which will be subjoined an inquiry into the relative mortality of the principal diseases of children in Glasgow, during the last thirty years; and the number who have died at various periods under ten years of age.

Dr. Smith and Mr. Sowerby will complete their work on English Botany by a general Index to the thirty-six volumes, which will be completed on the 1st of January, 1814. It is intended to arrange the names of the English plants contained in that work, which will amount to nearly 2600, in one part alphabetically, and in another part according to the Linnæan system, with such improvements as have been received during its publication. When the English Botany is completed, Mr. Sowerby hopes to be able to publish his Mineral Conchology every month. The British and Exotic Mineralogy will, in all probability, be finished in the course of the next year.

Dr. John Mason Cox, of Fish Ponds, near Bristol, is preparing an enlarged edition of his Practical Treatise on Insanity.

The second half of Volume VI. of the Edinburgh Encyclopædia, by Dr. Brewster, is just ready. Its leading articles are Chronology, Circle, Civil Architecture, Cloth Manufacture, Coining Machinery, Cold, &c.

Capt. Flinders's Account of his Voyage to Terra Australis, in his Majesty's ship Investigator, is preparing for publication, by authority of the Board of Admiralty.

Mr. John Whitsed, of Peterborough, will shortly publish Practical Remarks on Diseases resembling Syphilis, with cases.

⁂ Early Communications for this Department of our Journal will be thankfully received.

Article XVI.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer. Max.	Barometer. Min.	Barometer. Med.	Thermometer. Max.	Thermometer. Min.	Thermometer. Med.	Evap.	Rain.	
5th Mo.										
May 23	W	29·59	29·55	29·570	59	49	54·0			☾
24	N W	29·70	29·55	29·625	60	49	54·5			
25	N W	29·76	29·65	29·705	61	49	55·0			
26	W	30·05	29·76	29·905	57	41	49·0			
27	N W	30·10	30·05	30·075	60	42	51·0			
28	Var.	30·10	29·96	30·030	72	42	57·0			
29	N E	30·06	29·96	30·010	76	53	64·5		—	●
30	S	29·97	29·90	29·935	73	56	64·5	·65	4	
31	N W	30·00	29·97	29·985	78	59	68·5		2	
6th Mo.										
June 1	E	29·97	29·83	29·900	85	50	67·5			
2	N E	29·95	29·83	29·890	84	54	69·0			
3	N W	30·17	29·95	30·060	72	51	61·5			
4	N W	30·17	29·88	30·025	65	51	58·0	·75		
5	N W	29·88	29·82	29·850	58	48	53·0			☽
6	N E	29·73	29·67	29·700	56	47	51·5			
7	N E	29·80	29·73	29·765	71	43	57·0			
8	N E	29·73	29·51	29·620	75	48	61·5			
9	N W	29·53	29·43	29·480	75	54	64·5	·48	—	
10	N W	29·83	29·53	29·680	75	48	61·5			
11	S	29·83	29·74	29·785	76	52	64·0			
12	S E	29·98	29·74	29·860	77	45	61·0			
13	W	30·02	29·97	29·995	75	51	63·0	·48	5	
14	S W	30·07	29·78	29·925	67	52	59·5			○
15	S W	29·98	29·78	29·880	67	46	56·5			
16	N W	30·04	29·98	30·015	63	44	53·5			
17	Var.	30·09	30·04	30·065	61	43	52·0			
18	N	30·15	30·09	30·120	61	39	50·0			
19	N	30·16	30·15	30·155	57	37	47·0			
20	N E	30·20	30·16	30·180	60	42	51·0	·42	·65	
		30·20	29·43	29·889	85	37	57.93	2·78	1·06	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Fifth Month. 30. A shower, p. m. Thunder to the westward. 31. Fine day: some thunder clouds appeared: the evening twilight was brilliant, and tinged with orange; the new moon was conspicuous, and there fell much dew.

Sixth Month. 1. *Cumulus*, *Cumulostratus*, and *Cirrostratus* clouds: the sunset was cloudy, with an orange tint. 2. At sunset, *Cirrostrati*, mixed with haze, in the S. while in the N. there appeared large *Cirri*, more elevated, and finely tinged with red. 4. Windy: cloudy till evening. 5. Clear, a. m.: afternoon cloudy and windy. 8. Windy: at sunset, *Cumuli*, with the *Cirrostratus* attached: much orange in the evening twilight. 9. A shower early: cloudy; dripping. 10. p. m. Large elevated *Cirri*. 11. *Cumulostratus*, a. m.: in the afternoon *Cirri* in abundance, lowering and thickening. 12. *Cirri* tinged red, early in the morning: before eight a. m. it was overcast, and rain fell. 13. a. m. Cloudy: a shower at evening. 14—20. Occasional heavy showers, with wind.

RESULTS.

Prevailing Winds, Northerly.

Barometer: greatest observed height ...30·20 inches;
Least ....29·43 inches;
Mean of the period ......29·889 inches.
Thermometer: greatest height ....85°
Least ....37°
Mean of the period....57·93°
Evaporation, 2·78 inches. Rain, 1·06 inches.

Tottenham, L. HOWARD.
Sixth Month, 23, 1813.

*** For the greater part of the observations in the Barometer and Thermometer during this period I am indebted to my friend John Gibson, of Stratford.

ANNALS

OF

PHILOSOPHY.

AUGUST, 1813.

ARTICLE I.

Biographical Account of M. Lavoisier. By Thomas Thomson, M.D. F.R.S.

I HAVE drawn up the following account, not with any idea that the life and unfortunate fate of Lavoisier are unknown to my readers; but because I conceived that a particular account of the writings of that eminent philosopher must be acceptable to every person who is interested in the progress of science, and because I suspect that a much greater number of persons are acquainted with the name of Lavoisier than with the different dissertations which he published, or the particular improvements which he introduced into the science of chemistry. My knowledge of the few facts stated below respecting the life of Lavoisier is derived from an *eloge* of him published by Lalande about the beginning of the present century. This eloge was translated into English; and, unless I am mistaken, was inserted in the Philosophical Magazine.

M. Lavoisier was born in Paris, in 1743; and his father, who was opulent, spared no expense in his education. He displayed a decided taste for the sciences at a very early age; and the progress which he made in them was very rapid. When only twenty-one years of age, he obtained the prize offered by government for the best essay on the method of lighting the streets of Paris. In the year 1768 he was admitted a member of the Academy of Sciences. He was now become conscious of the power which he possessed, and burnt with an ardent zeal to distinguish himself in some department of science; but he hesitated for some time to what branch of science he should devote his chief attention. Mathematics first caught his fancy, and were enlisting him under their banners, when the brilliant discoveries of Dr. Black and his followers in the pneumatic career came to his knowledge, and determined him to make choice of chemistry. In 1774 he published a volume of essays, in which he gives an historical detail of the labours of his predecessors in this department of the science, and confirms the theory of Dr. Black by his own experiments.

His great wealth, his situation, his excellent education, his mathematical precision, his general views, and his persevering industry, all contributed to ensure his success. A comparison of the Stahlian theory with the discoveries in chemistry made by his contemporaries in Britain and Sweden, soon satisfied him of its insufficiency to explain the phenomena of the science. The opinion on the subject which he finally adopted, namely, that *phlogiston* is a creature of the imagination, and that combustion is occasioned by the union of *oxygen* with the burning body, must have been brought into a definite form after the year 1774, because it was not till that year that oxygen gas was discovered by Dr. Priestley; and it is too obvious to require any proof that his opinion respecting the combination of oxygen in cases of combustion could not precede his knowledge of the existence of that principle. This I conceive to be a sufficient answer to the remarks of M. Berthollet, and several other gentlemen, upon an observation of mine in the first volume of my *System of Chemistry*, that Bayen's paper on mercury, published in 1774, first suggested to Lavoisier his theory of combustion. This observation was borrowed from M. Delametherie, whose history of the progress of the antiphlogistic theory published in the *Journal de Physique*, as far as I am able to judge, is accurate.

Be this as it may, there can be no doubt that he was in possession of the first hint of his theory at least as early as the year 1775. He revolved it in his mind with the utmost perseverance for ten years, repeated all the important chemical experiments of others, and contrived a great many of his own with reference to his peculiar theory, and at last succeeded in demonstrating by indisputable experiments a theory almost the reverse of that of Stahl; namely, that we have no evidence of the existence of the substance called *phlogiston*, and that in all cases of combustion *oxygen unites* with the burning body. If the opinion lately advanced by Sir Humphry Davy, and which, to me at least, appears founded upon very strong, though not perhaps decisive evidence, be adopted; namely, that *chlorine* or *oxymuriatic acid* is a simple substance, and muriatic acid a compound of hydrogen and chlorine, we must in that case modify the theory of Lavoisier, and admit combustion to take place during the combination of chlorine with combustible bodies, as well as oxygen. Indeed, it seems established that combustion takes place in all cases of very rapid combination, whenever one of the combining substances changes its state, and from being gaseous or liquid becomes solid; so that the Lavoisierian theory of combustion is now, we believe, universally abandoned; but this does not detract from the merit of Lavoisier, nor diminish the value of his discoveries.

In the year 1789 M. Lavoisier published an exposition of his theory in a work to which he gave the title of *Elements of Chemistry*, a work which cannot be praised too highly, whether we consider the arrangement or the matter.

M. Lavoisier under the old French Government was one of the farmers-general of the revenue, a situation usually attended with much unpopularity. About the commencement of the French revolution, he was appointed to superintend the saltpetre works, which exist in considerable numbers in France. As long as the management of affairs continued in the hands of men of respectability and education, neither his income nor his situation in life was injured; but after the destruction of the Brissotin faction, when Robbespierre and his vandals assumed the supreme power, all the institutions of education, all literature and science, were immediately proscribed, and the object of the ruling faction seemed to be to bring back France to a state of absolute barbarism. During this dreadful period Lavoisier became apprehensive that he would be stripped of his property, and he informed Lalande that if such an event took place he would support himself by commencing apothecary; but his days were cut short by the bloody policy of the ruling faction. He was accused of defrauding the public revenue; and in those dreadful days accusation and condemnation were scarcely ever separated. He was thrown into prison, and suffered on the scaffold on the 8th of May, 1794, in the fifty-first year of his age. He was employed at the time in a set of experiments on respiration, and requested a delay of a few days in order to finish his observations; but he was answered, that the Republic had no occasion for chemists, and hurried out to immediate execution.

Let us now take a view of the numerous writings of Lavoisier; by which he secured to himself so high a reputation, and produced so great a change in the science of chemistry.

1. His first work, as far as I am acquainted with his writings, was a volume of essays, physical and chemical, published in the year 1774. This volume, which was translated into English by Mr. Henry, of Manchester, and into German by Mr. Weigel, is divided into two parts. In the first part he gives a minute and pretty accurate historical detail of the discoveries made in

pneumatic chemistry before his publication. He states the facts ascertained by Van Helmont, Boyle, Hales, Boerhaave, Stahl, Venel, Black, Saluces, Macbride, Cavendish, and Priestley; and gives us an account of Meyer's theory of calcination, and the cause of causticity in lime and alkalies, the defence of this theory by Crans and de Smeth, and its refutation by Jacquin. In the second part he relates a number of experiments of his own, chiefly relative to Dr. Black's theory of the difference between quick-lime and limestone, and of the cause of the causticity in alkalies. He demonstrates the existence of carbonic acid in the mild alkalies, and in calcareous spar; shows that it has the property of combining with the alkaline and earthy bases, and with metallic oxides. He ascertains the effect of this gas upon animals and upon combustion, and shows that water absorbs it. His experiments, though satisfactory as to the truth of Dr. Black's theory, are not to be compared to the preceding experiments on the same subject made by Mr. Cavendish, which are greatly superior both in point of accuracy and value. He terminates the volume by some experiments on the combustion of phosphorus, in which he shows that phosphorus absorbs a portion of the air, that it will not burn without air, and that it will not become acid without absorbing air. He shows the same thing with respect to sulphur, and he says that gunpowder will not burn in vacuo. How he fell into this mistake it is not easy to say. The contrary had been proved long before by Haukesbee, Robins, &c. He made some experiments on air, in which phosphorus has burnt out, and shows that it is not the same as carbonic acid gas. From these observations we see that he was on the way to his particular theory of combustion; though the ignorance of oxygen, or of the nature of air, prevented his views from being either steady or correct.

Though I have mentioned this volume of essays in the first place, because it was published by itself, yet it was not the first of M. Lavoisier's works. His other dissertations were published in the Memoirs of the French Academy, in the Journal de Physique, and in the Annales de Chimie. I shall notice them as nearly as possible in the order of their dates, though this is attended with considerable difficulty; because the Memoirs of the Academy were often published, and even contain papers written several years after the date upon the title page, and abstracts of his papers sometimes made their appearance in the Journal de Physique before they were published in the Memoirs of the Academy. I may therefore now and then commit an error with respect to date; but in most cases this is immaterial.

2. His first paper in the Memoirs of the Academy is upon *Gypsum*. It will be found in the volume for 1768. He gives an account of the properties of gypsum at great length, and

details his experiments upon its composition. In these he had been anticipated by Margraaf, who in the Berlin Memoirs for 1750 had shown that gypsum is a compound of sulphuric acid, lime, and water.

3. In the Memoirs of the Academy for 1770 are two very elaborate papers by Lavoisier *on the Nature of Water, and on the Experiments which were supposed to prove that it might be converted into an Earth*. In the first he gives a very full account of the experiments and opinions of preceding chemists on this subject. He states the experiments of Van Helmont, Boyle, Triewald, Miller, Eller, Gleditch, Bonnet, Kraft, Alston, Wallerius, Beccher, Stahl, and Margraaf. There is some confusion in this historical detail. The experiments of Boyle, Eller, and Margraaf, lead directly to the conclusion that water by long digestion in glass vessels may be converted into earth; while the other experimenters endeavoured to prove that it might be converted into all the constituent parts of vegetables. In the second part he gives us the result of his own experiments, from which it appears that warm water has the property of decomposing glass; and that the earth obtained by Boyle, Eller, and Margraaf, was merely a portion of the silicious earth of the glass vessels in which the experiments had been performed. Scheele came to the same conclusion, and details his experiments in his Treatise on Fire, published in 1777.

4. In the Memoirs of the Academy for 1771 we find his *Calculations and Observations on the Project of erecting a Steam Engine to supply Paris with Water*. This elaborate dissertation being merely of a local nature, we do not consider it necessary to give any details respecting it.

5. *A Memoir on the Use of Spirit of Wine in the Analysis of Mineral Waters.* Mem. Par. 1772. In this paper he details his experiments on the solubility of different salts in alcohol of different strengths. He gives many valuable directions about the method of proceeding in the analysis of water; and relates various experiments on the constituents of sea-water, and on other particulars connected with this branch of chemistry. Though this paper is not equal in importance to the dissertation published soon after by Bergman on the same subject, it possesses, notwithstanding, considerable value. Hence it is surprising that it has been so completely overlooked by succeeding writers on the subject. The greatest fault in it is the not giving the specific gravity of the alcohol employed.

6. *Two Memoirs on the Destruction of the Diamond by Fire.* Mem. Par. 1772. Published, I believe, in 1776. The first of these memoirs contains, according to the plan adopted by Lavoisier, an historical detail of the experiments made by others

on the combustibility of the diamond; while in the second memoir he makes us acquainted with his own experiments on the subject. He shows that the diamond is a combustible substance; and that when it is burnt, a portion of the same gas (carbonic acid) is formed, which makes its appearance after the combustion of charcoal. This paper is interesting, not only for this curious discovery, but because it seems to have led Lavoisier to those experiments which afterwards made him acquainted with the constituents of carbonic acid.

7. *Report made to the Academy of Sciences by MM. Fougeroux, Cadet, and Lavoisier, of an Observation communicated by M. l'Abbé Bachelay respecting a Stone supposed to have fallen from the Clouds during a Storm.* Jour. de Phys. ii. 251. This paper is curious. The opinion that the stone fell from the skies is treated with contempt. There is an imperfect analysis of it, from which it appears that it contained sulphur, iron, and earth. Hence there is every reason to believe that it was similar to the meteoric stones, with which at present we are so well acquainted.

8. *Observations communicated to the Royal Academy of Sciences on a singular Effect of Thunder.* Jour. de Phys. ii. 310.

9. *A Letter to the Author of the Recueil sur l'Atlas Mineralogique de la France.* Jour. de Phys. ii. 372. This letter contains a variety of mineralogical remarks on France, but too imperfect to deserve to be transcribed.

10. *Experiment on the Freezing of Water.* Jour. de Phys. ii. 510. This is a verification of Dr. Black's theory of latent heat, which had been already made known in France by Desmarets, and in Sweden by Wilke.

11. *Observations on some Circumstances respecting the Crystallizing of Salts.* Jour. de Phys. 1773, p. 10. These relate to some peculiarities respecting the crystallization of sulphate of soda, supposed by Baumé to be owing to some alkalies lying at the distance of several feet. Lavoisier showed that the inference was inaccurate.

12. *Analysis of the White Lead Ore of Poulawen in Lower Britany, by MM. Bourdelin, Malouin, Macquer, Cadet, Lavoisier, and Baumé.* Jour. de Phys. iii. 346. They demonstrated that the opinion entertained by Sage, that this ore was a muriate of lead, is incorrect. From the experiments which they made upon it there can be little doubt that the ore examined was carbonate of lead; though there is no description sufficient to make us acquainted with the characters which it possessed.

13. *First Essay of the great Burning Glass of M. Trudaine established in the Garden of the Infanta in October, 1774; by M. Trudaine, de Montigny, Macquer, Cadet, Lavoisier, and*

Brisson. Mem. Par. 1774. Published, I believe, in 1778. This paper is curious, as it details the effect of a violent heat upon a variety of bodies.

14. *Memoir on the Calcination of Tin in close Vessels, and on the Cause of the Increase of Weight which that Metal experiences during that Process.* Mem. Par. 1774. Published, I believe, in 1778. This is a very important paper. It consists in the repetition of an experiment formerly made by Boyle. Lavoisier demonstrated that a portion of the air combined with the tin during the calcination, and occasioned the increase of its weight. This important fact, combined with some experiments made about the same time by Bayen on mercury, enabled him to give a new and satisfactory explanation of the calcination of metals. This paper, therefore, may be considered as constituting the basis of the Lavoisierian theory.

15. *Memoir on the Nature of the Principle which combines with the Metals during their Calcination, and which increases their Weight.* Mem. Par. 1775. Published, I believe, in 1778. In this paper he farther developes the nature of calcination, and explains some of the properties of oxygen gas. As far as I recollect (for I have not the volume of the Academy at hand), Dr. Priestley's name never occurs in the whole of this paper, nor is any notice taken of the striking peculiarities of oxygen gas already described by Priestley. It is somewhat difficult to reconcile this conduct of Lavoisier with candour; for Dr. Priestley had published his account long before this paper met the public eye: and he informs us himself that while he was in Paris in 1774 he showed Lavoisier the way of procuring oxygen gas from red precipitate, and exhibited to him its most remarkable property of supporting combustion better than atmospheric air. Now this was the very process employed by Lavoisier to procure oxygen gas.

16. *Report made to the Academy of Sciences on Rouge.* Jour. de Phys. tom. vi. p. 322. This is a kind of historical account of the red paint used for the face. It is known to be extracted from the blossom of the *carthamus tinctorius.*

17. *Memoir on the Existence of Air in Nitrous Acid, and on the Method of Composing and Decomposing that Acid.* Mem. Par. 1776. Published, I believe, in 1780. In this paper Lavoisier gives an account of the experiments of Priestley, and he shows experimentally that nitric acid is a compound of nitrous gas and oxygen gas.

18. *On the Combustion of the Phosphorus of Kunkel, and on the Nature of the Acid which results from that Combustion.* Mem. Par. 1777. Published, I believe, in 1781. This paper may be considered as the basis of Lavoisier's theory respecting the constitution of acids. He shows that phosphoric acid is a

compound of phosphorus and oxygen. He describes the properties of phosphoric acid, and of the salts which it forms with the different bases. Till this paper appeared, phosphoric acid was unknown to the chemical world.

19. *Analysis of some Waters brought from Italy by M. Cassini, jun.* Mem. Par. 1777. Published, I believe, in 1781. These waters were found to hold in solution a considerable quantity of alum and sulphate of iron, and to contain a great excess of sulphuric acid.

20. *Experiments on the Ash employed by the Saltpetremakers of Paris, and on its Use in the Preparation of Saltpetre,* Mem. Par. 1777. Published, I believe, in 1781. Lavoisier found this ash to yield the following salts:—

Sulphate of potash.
Sulphate of soda.
Common salt.
A salt in needles, nature unknown,
Sulphate of lime.

Besides a quantity of combustible and insoluble earthy matter.

21. *Experiments on the Respiration of Animals, and on the Changes produced in the Air by being drawn into the Lungs,* Mem. Par. 1777. Published, I believe, in 1781. This is one of the most celebrated of all Lavoisier's papers. He gives a history of the observations formerly made on respiration by Hales, Cigna, and Priestley; but takes no notice of the discovery of the evolution of carbonic acid gas by breathing made by Dr. Black. In this paper he shows that oxygen disappears, that carbonic acid and a quantity of water appears; and he endeavours to assign the proportions of these changes; but he afterwards altered his opinion respecting them.

22. *Memoir on the Combustion of Candles in Atmospheric Air and in Oxygen Gas,* . Mem. Par. 1777. Published, I believe, in 1781. He shows that a portion of the oxygen is converted into carbonic acid gas, and that the candle goes out long before the whole of the oxygen of the air is abstracted.

23. *Memoir on the Solution of Mercury in Sulphuric Acid, and the Decomposition of that Acid into Sulphurous Acid Gas and Oxygen Gas.* Mem. Par. 1777. Published, I believe, in 1781. This was the first attempt to ascertain the composition of sulphuric acid, and to point out the real difference between it and sulphurous acid.

24. *Experiments on the Crystallization of a Fluid Alkali by Electricity.* Jour. de Phys. tom. x. p. 104.

25. *Experiments on the Combination of Alum with Charry Matters, and on the Changes produced in the Air in which Pyrophorus is burnt.* Mem. Par, 1777. Published, I believe, in

1781. He shows the formation of carbonic acid gas, and the diminution of the oxygen of the air.

26. *Memoir on the Vitriolization of Martial Pyrites.* Mem. Par. 1777. Published, I believe, in 1781. He shows that during the process the oxygen of the air is absorbed.

27. *On the Combination of the Matter of Fire with evaporable Fluids, and on the Formation of the elastic aeriform Fluids.* Mem. Par. 1777. In this paper he lays down the doctrine that heat combines with bodies, and changes them into elastic fluids; a doctrine obviously founded on Dr. Black's theory of latent heat, though Dr. Black's name never occurs in the paper.

28. *Experiments made by Order of the Academy on the Cold of the Year* 1776; *by MM. Bezout, Lavoisier, and Vandermonde.* Mem. Par. 1777.

29. *Memoir on Combustion in general.* Mem. Par. 1777. In this essay, published in 1781, he gives us the first distinct account of his theory of combustion, and combats the Stahlian doctrine of phlogiston.

30. *Analysis of the Water of the Dead Sea*; *by MM. Macquer, Lavoisier, and Sage.* Mem. Par. 1778. Published, I believe, in 1782. The result of this analysis will be found in the first number of the *Annals of Philosophy.*

31. *Experiments on a White Steatite, converted by Fire into an excellent Biscuit of Porcelain; by MM. Guettard and Lavoisier.* Mem. Par. 1778.

32. *Description of two Coal Mines situated at the Foot of the Mountains of Voyes, the one in Franche Comté, the other in Alsace, with some Experiments on the Coal which they yield*; *by MM. Guettard and Lavoisier.* Mem. Par. 1778.

33. *General Considerations on the Nature of Acids, and on the Principles of which they are composed.* Mem. Par. 1778. In this paper he lays down his doctrine, that all acids contain oxygen, and that oxygen is the acidifying principle.

34. *Report made to the Academy on the Gold which may be extracted from the Ashes of Vegetables*; *by MM. Macquer, Cadet, Lavoisier, Baumé, Bouquet, and Cornette.* Mem. Par. 1778. This report was read to the Academy on the 21st of August, 1779. It refuted the opinion advanced by Sage, and some other chemists, that the ashes of vegetables yielded a considerable quantity of gold.

35. *Memoir on some Fluids which may be obtained in the aeriform State at a Temperature little superior to the mean Temperature of the Earth.* Mem. Par. 1780. Published in 1784. These fluids are ether, alcohol, and water. The experiments are not satisfactory.

36. *Second Memoir on the different Combinations of Phosphoric Acid.* Mem. Par. 1780. Published in 1784.

37. *Memoir on a particular Process for converting Phosphorus into Phosphoric Acid without Combustion.* Mem. Par. 1780. Published in 1784. This is the method, by the action of nitric acid, at present so well known.

38. *Memoir on Heat; by MM. Lavoisier and de la Place.* Mem. Par. 1780. This is the well known memoir in which the quantity of heat evolved by different bodies when burnt, &c. was measured by means of the calorimeter.

39. *Report made to the Royal Academy of Sciences on the Prisons, the 17th March, 1780; by MM. du Hamel, de Montigny, le Roy, Tenon, Tillet, and Lavoisier.* Mem. Par. 1780.

40. *Experiments on the Composition of Water.* Mem. Par. 1781. Published in 1784. This paper contains the result of Lavoisier's experiments on the composition of water. It is particularly valuable, on account of the historical details which it contains. Among other facts, he mentions, that before he began his own experiments he had been informed by Sir Charles Blagden that the experiment had been already performed by Mr. Cavendish, who had ascertained that water is a compound of oxygen and hydrogen gas.

41. *Experiments on the Decomposition of Water, showing that this Liquid is not a simple Substance, and pointing out a Method of obtaining Hydrogen Gas in abundance; by MM. Meusnier and Lavoisier.* Mem. Par. 1781. Published in 1784; and read to the Academy on the 21st of April, 1784. The method used was to pass water through red-hot iron. The iron was oxydated, and abundance of hydrogen gas was evolved.

42. *Experiments on the Electricity absorbed by Bodies during their Conversion into Vapour; by MM. Lavoisier and la Place.* Mem. Par. 1781. Published in 1784.

43. *Experiments on the Heat produced during the burning of different Combustibles.* Mem. Par. 1781. Published in 1784.

44. *Remarks on Scheele's Book on Fire and Air.* Mem. Par. 1781. Published in 1784. These remarks are chiefly confined to the phenomena of calcination and combustion.

45. *Observations on the Method of Illuminating Theatres.* Mem. Par. 1781. Published in 1784.

46. *Experiments on the Composition of the Acid called Fixed Air, or Calcareous Acid, and which I for the future shall call Carbonic Acid.* Mem. Par. 1781. Published in 1784. This is another of the most important of Lavoisier's experimental essays. He determines the composition of carbonic acid with much industry; and the proportions which he assigns, 28 carbon and 72 oxygen, are probably as near the truth as any other results hitherto offered to the public. In this paper he distinguishes *vital air* by the term *oxygen*, which it at present bears.

This essay, together with one of Cavendish on the same subject, was the principal weapon employed to overturn the notion of phlogiston advanced by Mr. Kirwan.

47. *Description of a Method to increase the Effect of Heat produced by Chemical Operations.* Mem. Par. 1782. Published in 1785. This was by means of a stream of oxygen gas, a method now too well known to require any description here. Lavoisier describes the apparatus which he employed for the purpose.

48. *Experiments on the Effect produced by a very high Temperature on the Precious Stones.* This consists in a detail of the experiments made by means of the apparatus described in the preceding paper.

49. *On the Union of Nitrous Gas with Common Air.* This is a eudiometrical paper, in which Lavoisier endeavours to calculate from the mixture of nitrous gas and common air, and the diminution of bulk, the quantity of oxygen gas contained in common air. It is needless to observe that it is far inferior to Mr. Cavendish's paper on the same subject.

50. *General Observations on the Solution of the Metals in Acids.* Mem. Par. 1782. Published in 1785. This is another of the important dissertations by which Lavoisier established his theory. He makes use of the decomposition of water with great address and ingenuity. Indeed, I consider this as the most profound of all Lavoisier's essays. He commits occasional mistakes, but his reasoning on the whole is accurate.

51. *Experiments on the Precipitation of one Metal by another.* Mem. Par. 1782. Published in 1785. This is also a very curious paper. He gives the true theory of these precipitations, and endeavours from them to determine the proportion of oxygen which unites with the different metals. Though all his results are wrong, his table is still curious, as it exhibits the first attempt to determine this very important point.

52. *On the Affinity of Oxygen for the different Bodies with which it can unite.* Mem. Par. 1782. Published in 1785. This may be considered as a kind of treatise on affinity. Considerable ingenuity is displayed in determining the affinity of different bodies for oxygen; and though the table given is not accurate, it constituted a valuable groundwork for succeeding experimenters.

53. *On the Combination of Oxygen with Iron.* Mem. Par. 1782. Published in 1785. These elaborate experiments cannot be considered as very successful.

54. *On the Nature of the elastic Fluids disengaged during the Fermentation of Animal Substances.* Mem. Par. 1782. Published in 1785. The chief gas obtained during his experiments was carbonic acid gas.

55. *New Observations on the Increase of Weight which*

Sulphur and Phosphorus undergo when burnt. Mem. Par. 1783. Published in 1786.

56. *Remarks on Phlogiston. An Exposition of the Theory of Combustion and Calcination made known in* 1777. Mem. Par. 1783. Published in 1786. This is a full exposition of his own theory, and a refutation of the Stahlian theory. By this time Lavoisier had been joined by Berthollet and Fourcroy, and had formed the design of propagating his opinions.

57. *Experiments on the Effect of a Fire supported by Oxygen Gas on the Weight and Fusion of Mineral Bodies.* This is a continuation of a subject already begun in a preceding paper.

58. *Memoir on the Manufacture of Saltpetre.* Ann. de Chim. xv. 225. It is generally believed that Lavoisier was mistaken in his opinion respecting the great quantity of saltpetre volatilized during the boiling of the ley.

59. *Result of some Experiments on Agriculture, and Reflections on their Connection with Political Economy.* Ann. de Chim. xv. 297.

These are all the papers of Lavoisier with which I am acquainted. It is probable, considering the dreadful period during which he terminated his career, that he may have published others with which I am unacquainted. His Essay on the Chemical Nomenclature, and his Elements of Chemistry, are too well known to require being particularized. I do not notice the posthumous volume of his essays published about six years ago in Paris by his widow, because I have never had an opportunity of seeing the book. It is chiefly historical, and will be more curious by determining the objects which Lavoisier aimed at, than the dates of his speculations, which are sufficiently determined from his original publications.

ARTICLE II.

Experiments on the absolute Magnetic Declination, and on the Amount of the Horary Variations of Needles in the same Place and Time, according as the Magnetic Fluid is differently distributed in their Interior. By Dr. Schübler, of Stuttgardt.*

The following experiments were made with simple needles, three or four feet in length, suspended by a fine silk thread, in an apparatus which screened them from the action of the air. The magnet which communicated to them the magnetic virtue was in the shape of a horse-shoe, and usually weighed about four pounds.

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* Translated from the *Journal de Physique* for September, 1812, vol. lxxv. p. 178. I have been induced to translate this paper by the singularity of the results which it exhibits. Though far from convinced of their accuracy, I think they are entitled to a careful repetition, in order to ascertain whether the state of the needle itself has any effect upon the daily variation.

Exper. I.

I took a new needle, on which no magnetic experiments had been made, and converted it into a magnet in the ordinary way, by the double and single touch. It exhibited the small horary variations of the ordinary needle. I took a second needle, perfectly similar to the first; but instead of communicating to it magnetism in the usual way, I placed the south pole of the magnet on the middle of the needle, and drew it ten times to one of the ends of the needle without returning back again. I then drew it in the same way ten times towards the other end; and then by means of a small sensible needle determined the poles of this long needle, to which magnetism had been communicated in the peculiar way just described. Its two ends were north poles; the middle was a south pole (as might have been foreseen from the manner of communicating the magnetic virtue). A more attentive examination presented the following disposition of its magnetism:—

$$\begin{array}{c} \mathrm{o} \qquad -\mathrm{m} \qquad \mathrm{o} \\ +\mathrm{m} \longleftarrow\!\!-\!\!-\!\!-\!\!-\!\!-\!\!-\!\!\longrightarrow +\mathrm{m} \end{array}$$

This needle underwent a daily variation of 40 or 50 minutes, and even of 60 minutes, when the sky was serene; while an ordinary needle varied only 10 or 12 minutes per day. In February it moves towards the east at nine in the morning, and in the beginning of April at half-past seven in the morning. It moves towards the west between two and three in the afternoon, and with such rapidity as to alter 8 or 10 minutes in an hour. From three to nine it inclines again towards the east, and then returns again towards the west. Hitherto I have observed these variations to be greatest when the sky is serene, and smallest when cloudy or rainy. I have repeated the experiment with various needles treated in the same way, and have always obtained the same declination.

Exper. II.

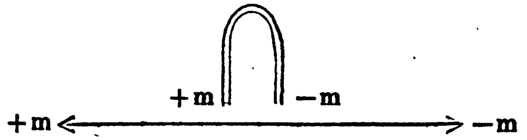
I took another needle similar to the former. I placed the south pole of the magnet upon its middle, and drew it ten times to the extremity of the needle inclined towards the north, while the other half of the needle remained untouched. This, of course, was the same kind of operation as the preceding. The needle, as before, exhibited great daily oscillations. An attentive examination showed me that this needle possessed two north poles, and that of the two which exhibited the most energy turned towards the north. In this respect the needle coincided with that of the preceding experiment.

Exper. III.

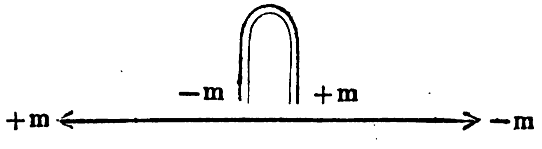
I took the needle of Experiment II. and placed on its middle the north pole of the magnet, and drew it ten times in the direction of the pole turned towards the south. The half turned towards the north remained untouched. The result was, that the pole previously turned towards the south became a south pole; the other pole became a north pole. The needle thus treated exhibited the common diurnal variation. It appeared, however, to be somewhat less regular than usual.

Exper. IV.

I took a needle of new iron, and communicated to it the magnetic virtue in the ordinary way, by the double touch. It had a north and south pole, and in the midst a point of indifference. It exhibited the usual diurnal variations of the common needle. In communicating the magnetism by the double touch, the north pole of the magnet was placed towards the pole of the same name of the needle, the south pole of the magnet towards the south pole of the needle, and the magnet was drawn ten times along the needle.



Upon a needle magnetized thus in the ordinary way, I placed a magnet in a position precisely the reverse, as is clearly seen in the following figure:—



I drew it along ten times towards the north pole of the needle, and as often returning to the middle, so that the south half of the needle was not touched at all. The half of the needle which had been touched acquired at its extremity a south pole, instead of the north pole which it formerly had. The needle turned round, and the touched end pointed towards the south.

$$-m \overset{+m}{\longleftrightarrow} -m$$

This needle presented a remarkable phenomenon. All the other needles that had been magnetized placed themselves in the magnetic meridian (which points 18° or 20° towards the west); but this needle turned its poles precisely to the north and south, its direction coinciding exactly with the meridian line of Stuttgardt. (Its true position and its exact deviation from the meridian line require, in order to be determined with precision, a more numerous set of experiments than I possess.) The oscillations of this needle were also extraordinary; they extended from 2° — 45′ to 3°; but they proceeded, like the others, in the evening and morning to their maximum towards the east, and about two o'clock to their maximum towards the west.

I afterwards communicated to this needle magnetism in the ordinary way by the double touch, and observed it in the same circumstances. It then assumed the direction of the magnetic meridian; the extraordinary oscillations disappeared, and the small oscillations of the common needle again made their appearance.

Exper. V.

To obtain the result of the fourth experiment in a more simple manner, I took another iron needle, placed on its middle the north pole of the magnet, and touched it, as in the first experiment, drawing it ten times towards one pole, and ten times towards the other, without returning. Both the poles (as I expected) were south poles, and in the middle of the needle there was a north pole. This needle took the position of the magnetic meridian, and not the true meridian, as was the case with the needle with two south poles in the fourth experiment. Its daily oscillation amounted to 50 or 60 minutes, being similar in this respect to the needle with two north poles of the first experiment, with this difference, that at ten in the morning it began to oscillate towards the west, while the needle which had two north poles oscillated at that time towards the east, but its maximum west was at two o'clock.

Two other needles, magnetized in the same way, exhibited exactly the same phenomena.

Exper. VI.

I took another new needle, and communicated to it the magnetic virtue exactly in the manner described in the fourth experiment.

This needle took the direction of the true meridian, and not of the magnetic meridian. Its oscillation from six in the morning till two o'clock (even in cloudy weather) usually amounted to three degrees. I observed its direction for three weeks. I have not yet succeeded in finding an explanation of a result so extraordinary as that of the fourth and sixth experiments.

Article III.

Description of a Compass for accurate Observations on the Magnetic Variation. By Col. Mark Beaufoy.

(With a Plate.)

The principal part of this instrument, viz. its needle, with the box and divided arch, to measure the angle of variation, is very similar to the instrument in the possession of the Royal Society; but it is greatly improved by the addition of a small transit telescope, which very readily and accurately determines the true meridian, or the zero from which the degree of the variation is to be measured. This addition renders it particularly well adapted for travellers who wish to make accurate observations in different parts of the globe, as it contains every thing necessary for the observation within itself; and as every part admits of adjustment, by reversing, it may be depended upon for accuracy.

Plate IX. fig. 1, contains a representation of the whole instrument in perspective, explaining all its parts at once. The box containing the needle is not fixed, as in many compasses, but turns horizontally on a centre, and has an index fastened to it, pointing to a divided arch, on the brass frame on which it turns; and the method of observing is to move the box till a line drawn on it points exactly to the end of the needle, which being done the angle of variation is shown by the divisions on the arch. F F is a mahogany board, which is the support of the whole instrument; it stands on the points of three screws, G, H, I, by which it can be levelled. Above this is a flat plate of brass, E E, attached to the board by a centre pin, and resting upon three studs, projecting from the board, to ensure its having a perfect bearing, whilst it admits of a small horizontal motion round the centre by means of the screw W X. The plate E E has the divided arch, *m*, fixed upon one end of it; and a vernier, D, traverses against the divisions; it is divided upon a projecting part of a brass plate, C C, which moves on the common centre pin of the plate E, and also the needle A A B. The plate C C has two segments of brass at each end of the needle, and these have the centre

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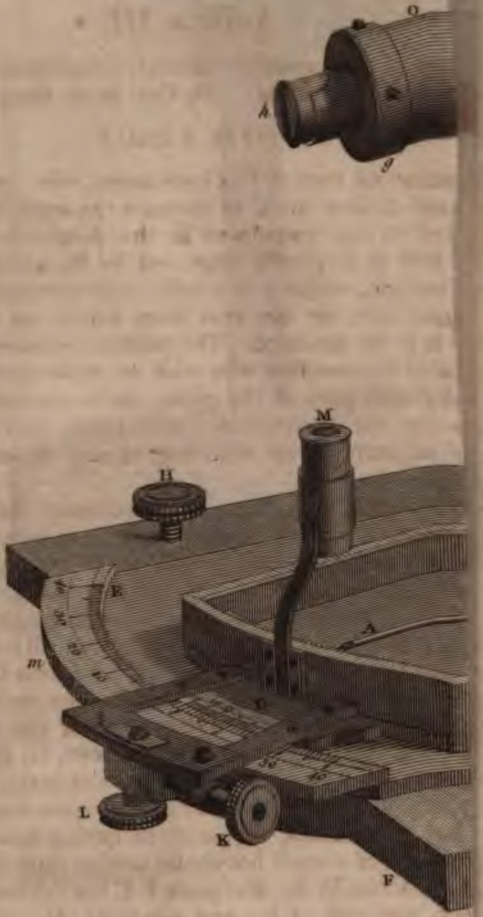

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lines drawn upon them, which are brought to the points of the needle when the observation is made. A light brass box, *d d*, with a glass cover, is fitted over the plate C C, to preserve the needle from disturbance from the wind; it also supports a small double microscope, M, intended to assist in examining when the index line points exactly to the end of the needle; the stem of the microscope is fitted in a dovetail groove, and can be removed to the opposite end of the box at pleasure.

'The centre pin of the plates E and C terminates in an extremely fine point, on which the needle is suspended, having an agate cap, B, to diminish the friction as much as possible. The needle is of a cylindrical figure, about the five-hundredth of an inch in diameter, and ten inches long, and weighs 65½ grains, and is terminated by two conical points; the circular enlargement of the centre has a hole through it, for the reception of the brass socket, B, which has the agate cap fixed in it; the needle is, of course, provided with the usual apparatus for lifting it off the point when not in use, to avoid wearing the point of suspension. *a, a*, are two arms, screwed down upon the plate, carrying the vernier, D; they support a brass frame, *b*, which has the usual clamp screw, L, and tangent screw, R, the former to fasten it to the arch, *m*, and the latter to give a slow motion, and adjust the box for the observation.

The transit telescope, O P, is supported over the instrument by two pillars, N, N, fixed on the brass plate, E E, and having small frames or boxes, *f*, at the top, for the reception of the Y's, in which the pivots of the axis, Q, of the telescope are supported; this axis is conical, and is fixed exactly at right angles to the tube, O P, in the same manner as other transit instruments.

On the extremity of one of the pivots of the axis, a small divided circle, R, is fixed, and has an arm at the opposite side turning about on the centre, and provided with verniers to read the divisions on the circle; to this arm a small level, S, is attached; the whole forming a flying index to set the telescope at any required altitude, by setting the index at the proper division, and then moving the telescope till the bubble of the level, S, shews the index to be horizontal; the eye-piece, *h*, of the telescope has a small dovetail in it to admit a dark glass for observation of the sun; *g*, are the screws for adjusting the wires in the eye-piece as usual; the separate figure, T V, is a level to adjust the instrument.

Manner of adjusting the instrument previous to observation.— First place the feet, *k, l*, of the level, upon the brass plate, E E, in different directions, and bring it level by the screws, H, I, K; then apply the level upon the two pivots of the transit, the covers of the boxes, *f*, opening with hinges for this purpose, and the bottom of the feet, *k*, *l*, have notches to rest upon the pivots; if the axis does not prove level, one of the Y's must be elevated or depressed, by a screw in the farther pillar, N, until the level stands horizontally, and, reversing it end for end, proves every-thing to be correct.

Now set the vernier, D, at zero, and put an additional object glass, over the glass, P, of the telescope; and in this state the marks near A A, against which the needle reads, can be seen through it when directed to them; this proves the zero of the division to be exactly in the plane of the telescope motion; a small screw near Q will rectify it, if necessary, by moving the Y a small quantity in the box, *f*; the transit may be reversed in its Y's to rectify every thing.

The telescope is now to be adjusted to the meridian by the transit of the stars in the usual manner, the screws, W, X, being used to turn the whole instrument round when requisite; but after these observations have been once made in a satisfactory manner, distant marks should be set up, both north and south, and these will give the means of adjusting it at once, and in the day time.

When the instrument is properly placed, the taking of the observation is extremely simple. The needle being suffered to settle, the box is turned about on its centre till the mark comes very near the point of the needle; the clamp screw, L, is then fastened, and the screw, *k*, is employed to make the coincidence exact, in which the microscope, M, greatly assists the eye. The vernier, D, now shows the angle of variation. It is proper, after the needle has been once observed, to attract it with a piece of iron, and cause it to make a slight vibration. When it settles again, it will not in all cases come to the same point, because of the friction of the suspending point; it is therefore advisable to make this trial three or four times, and take the mean of the whole.

The instrument is placed on a pillar, in the open air, free from any iron, and is adjusted before every observation; the vernier is divided to half minutes, and if the meridian be not at a considerable distance, an advantage will be gained by diminishing the aperture of the object-glass by a pasteboard cap. I have found the needles should not exceed the weight mentioned, 65½ grains.

It may be worth while to remark, that this instrument will serve as a portable transit instrument for naval officers to examine the rate of their chronometers while on shore.

ARTICLE IV.

On an easy Method of procuring a very intense Heat. By A. Marcet, M.D. F.R.S. Physician to Guy's Hospital.

(To Dr. Thomson.)

DEAR SIR, *Russel-square, July* 8, 1813.

AMONGST the various improvements which have assisted the progress of natural science within the last twenty years, that of carrying on experimental researches upon very small quantities of matter, ought, perhaps, to occupy the first place. The advantages which often result from a minute scale of operations are too obvious to require any comment; and indeed the philosophical records of this country have, of late years, afforded innumerable instances of the utility of this mode of investigation.

You may, perhaps, think it worth while to point out to the readers of your valuable Journal an easy and convenient method, which occurred to me about a year ago, for producing, upon a small scale, a degree of heat which has, I believe, never been exceeded in intensity, unless it be by the magnificent voltaic battery lately exhibited by Mr. Children. The process in question simply consists in urging the flame of a lamp of spirit of wine by a current of oxygen gas. The apparatus most convenient for this purpose is a tin vessel, or gas holder, such as represented in Plate IX. Fig. 2, from which a small jet of oxygen gas is forced out, with some degree of violence, by the introduction of water, through a funnel of one or two feet in length. If a diamond be exposed to the flame of a lamp, thus acted upon by the jet of gas, it burns, and disappears in a few minutes. Platina wire of moderate thickness is instantly melted, and globules of this metal, weighing as much as 4 or 5 grains, can thus be obtained in quick succession.* During this process of fusion, a scintillation of the metal is observed, as if it was undergoing combustion; but this appears to be owing to minute particles of melted platina, which are simply dispersed by the intensity of the heat. Iron wire is burnt by this means with a degree of rapidity and brilliancy which even exceeds that of Ingenhouz's striking mode of burning iron wire in oxygen gas; and small needles of quartz are readily melted and vitrified by the same means.

I am not aware of this mode of excitement having been employed before. Intense degrees of heat had long been produced from the combustion of hydrogen gas by a jet of oxygen; but the process was attended with some inconvenience and difficulty, and the effects, I believe, fell short of those just described.

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* It was from globules obtained by this process, that Dr. Wollaston succeeded in drawing platina wire of extreme minuteness, as stated in the paper which he has lately given to the Royal Society on the subject.

I remain, dear Sir, yours, &c.

Alex. Marcet.

Article V.

Observations on Radiant Heat. By F. Delaroche, M.D.*

I propose in this memoir to state several propositions which appear to me capable of throwing some light on the theory of radiant heat; and which, I think, I have established by decisive experiments. These experiments, indeed, were made with sufficient care to prevent any doubts about their exactness; but I may be deceived in the conclusions that I deduce from them. In that case I shall readily acknowledge my error; nor shall I think that I have lost my labour if I draw, upon so interesting a subject, the attention of some philosopher more fortunate than myself, or better situated for examining it with accuracy.

First Proposition.—*Invisible Radiant Heat may in some Circumstances pass directly through Glass.*

Different philosophers, and particularly Mr. Leslie, conceive that they have proved the falsehood of this proposition; but the experiments of Professor Prevost, of Geneva, have lately established its truth in an incontestable manner. He obtained his result by separating the immediate effects of transmitted heat from those produced by the heating of the glass, by a process equally simple and ingenious; namely, by employing moveable screens of glass, which he renewed continually, and of course did not give them time to become heated. I have myself, since I became acquainted with the memoir of M. Prevost, made a great many experiments, which appear to me to prove the same thing. The nature of these will be stated in support of the second proposition.

Second Proposition.—*The Quantity of Radiant Heat which passes directly through Glass is so much greater relative to the whole Heat emitted in the same Direction, as the Temperature of the Source of Heat is more elevated.*

M. Delaroche shows, in the first place, by some ingenious experiments, that a thermometer (of the temperature of the surrounding atmosphere) exposed to the action of radiant heat rises in a given time, ceteris paribus, a quantity proportional to the rays which it receives. If a certain number of rays make it rise 1 degree, double the number will make it rise 2 degrees, triple the number 3 degrees, and so on. This being established, he placed two parabolic metallic mirrors at the distance of rather more than three feet from each other. In the focus of one mirror, he placed a thermometer; and in the focus of the other a hot body, gradually increasing in temperature. The thermometer was allowed to rise to its maximum without any screen. Then a screen of transparent glass was interposed, and the experiment repeated. Lastly, a screen of blackened glass was interposed, and the experiment repeated a third time. By the blackened screen all the radiant heat would be intercepted, and the effect on the thermometer would be owing to the rise in the temperature of the screen. Hence the rise of the thermometer when the blackened screen was used, subtracted from the same rise when the transparent screen was used, leaves the effect produced by the radiant heat passing through the glass. The following table shows the ratio between the rays passing through the clear glass, and the rays acting on the thermometer, when no screen was interposed, at different temperatures :—

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* Abridged from the *Journal de Physique*, vol. lxxv. p. 201.

Temperature of the Hot Body in the Focus.	Rays transmitted through the Glass Screen.	Total Rays.
357°	10	263
655	10	139
800	10	75
1760	10	34
Argand's lamp without its chimney	10	29
Ditto, with glass chimney	10	18

Third Proposition.—*The Calorific Rays which have already passed through a Screen of Glass, experience, in passing through a second Glass Screen of a similar nature, a much smaller Diminution of their Intensity than they did in passing through the first Screen.*

This proposition was proved by interposing, first one glass screen, and then two, and observing the difference in the effect. The experiments appear conclusive.

Fourth Proposition.—*The rays emitted by a hot Body differ from each other in their faculty to pass through Glass.*

This proposition is an obvious consequence from the third, which can hardly leave a doubt that the calorific rays (like those of light) are of different kinds.

Fifth Proposition.—*A thick Glass, though as much or more permeable to Light than a thin Glass of worse Quality, allows a much smaller Quantity of Radiant Heat to pass. The Difference is so much the less as the Temperature of the radiating Source is more elevated.*

This proposition is proved by experiments made with glasses of different thickness employed as a screen; and its truth is sufficiently established, if these experiments be accurate. This curious fact, that radiating heat becomes more and more capable of penetrating glass, as the temperature increases, till at a certain temperature the rays become luminous, leads to the notion that heat is nothing else than a modification of light, or that the two substances are capable of passing into each other.

Sixth Proposition.—*The quantity of Heat which a hot Body yields in a given Time by Radiation to a cold Body situated at a Distance, increases, cæteris paribus, in a greater Ratio than the excess of Temperature of the first Body above the second.*

This proposition being at variance with the opinion of Mr. Leslie, and of several other philosophers, M. Delaroche thought it necessary to establish it by a great variety of experiments. These experiments leave no doubt of the fact; though they are not sufficient to enable us to deduce the rate at which the increase takes place.

I must acknowledge that this proposition appears to me somewhat puzzling. One is at first sight disposed to account for it by the inaccuracy of the thermometer as a measure of heat; but M. Delaroche, aware of such an objection, has remarked, that if we adopt Mr. Dalton's opinion with respect to the thermometer, so far from removing the apparent anomaly, it would only serve to make it greater.

Article VI.

On the Instruments necessary for a Travelling Astronomer. By Col. Beaufoy.

(To Dr. Thomson.)

My dear Sir, *Hackney Wick, June* 17, 1813.

The following instruments, I think, a travelling astronomer, or naval man, should take with him. First: a good Hadley's sextant, of six inches radius, and which I would have divided into half minutes, and not nearer.

Secondly: an artificial horizon of quicksilver, with a glass roof that folds up, to prevent the wind disturbing the surface of the mercury, when the observation is to be made: there should also be a floating glass, ground truly parallel, to place on the surface of the quicksilver in windy weather, or in those places where the ground is liable to be shaken with carriages.

Thirdly: a pocket compass. The whole of these instruments may be readily packed up in a small leathern case, furnished with straps, to buckle over the shoulders similar to a knapsack.

The next instrument, and without which the others would be comparatively of little use, is a chronometer: a silver one would be the cheapest, the price being about forty guineas; and I would recommend Mr. Pennington, of Camberwell, to be the maker; and it is better to give the order at least twelve months before it is wanted, as chronometers accelerate when first made, and sufficient attention, generally speaking, is not paid to the compensation for cold; the same remark is frequently applicable to the want of adjustment when the chronometer is placed in different positions.

To the instruments already mentioned, may be added a telescope, furnished with a micronometer for observing the eclipses of the sun and moon, occultations of the stars by the moon, and the eclipses of Jupiter's satellites.

I am, my dear Sir, sincerely yours,

M. Beaufoy.

Article VII.

On Transition Rocks. By James Grierson, M.D.

(To Dr. Thomson.)

SIR,

In the last Number of your *Annals*, I read, with much interest, a paper entitled " Remarks on the Transition Rocks of Werner, by Thomas Allan, Esq. F.R S.E. In this paper, after paying some well-merited and proper compliments to the ingenuity and perseverance of Werner, in having furnished us with " means by which rocks may be described with some degree of accuracy," Mr. Allan observes, that " we cannot extend our unqualified approbation to the *systematic arrangement* he has introduced." Werner, he says, was confined within certain limits by the narrowness of his fortune and the circumstances of his situation, and therefore could not be expected to form a perfect system, and has drawn " conclusions that are more general than are warranted by the circumscribed field to which he was confined."

But does it follow, Sir, that because Werner has never personally visited every quarter of the globe, and seen with his own eyes every rock *in situ*, and the nature of its composition, he may not from what he has seen and actually did visit, as well as from the accounts and specimens transmitted to him by others, have discovered by his genius and penetration the true principle of arrangement on which the crust of the earth is formed. Certainly not. As well might Mr. Allan object to the botanical arrangement of Linnæus, because thousands of plants have been discovered in New Holland, and elsewhere, to which that great naturalist was an entire stranger. The arrangement of Werner, then, provided the phenomena of nature be found conformable to his views, is not to be rejected merely because he never travelled beyond the bounds of Germany.

I am not sure, Sir, whether Dr. Hutton ever saw more of the crust of the earth than Werner; and certainly he never saw the central parts of it, about which, notwithstanding, he has told us so many wonderful things. If Werner generalized too liberally from a "circumscribed field," certainly Dr. Hutton was not behind him here. His conclusions are of a far more extensive nature than those of Werner, and surely drawn from premises comparatively much less extensive.

It is observed, too, that the appearances of the country about Edinburgh (the residence of Dr. Hutton) are greatly more favourable to the theory adopted by that philosopher than those of almost any other part of the world that has been hitherto explored. If Werner, then, formed his theory from the appearances of the country around Freyberg, Dr. Hutton seems no less to have taken his from those around Edinburgh.

But it is not in this way that the merits or demerits of either theory are to be estimated. Which of the two is most useful? Which of them teaches us best to distinguish minerals from one another? Which of them is best confirmed by the observations of succeeding naturalists? These are the important inquiries.

With the wish of Mr. A. "that arrangements of this nature were less dictated by theory," we cordially agree; but when he talks about "the pupils of the Wernerian school being peculiarly fettered by an ideal necessity of supporting the principles of their master," and their "totally neglecting that minute detail which is alone capable of placing the student in a situation to draw conclusions of his own," we are tempted to exclaim with the poet, *Mutato nomine, de te fabula narratur*; for if there be a department in which the Wernerian system seems to excel all others, it is, I think, in the minuteness with which its details are conducted, and the happy power which it possesses of discriminating what others almost always, though really distinct, confound. Mr. Allan himself acknowledges (p. 419) that Dr.

Hutton applied the term *alpine schistus* in such a way as " to leave us quite uncertain with respect to the species of rock he meant." And this is far from being the only instance that might be easily adduced, not only from his writings, but also from those of more recent philosophers of the same school.

Mr. A. justly observes, that the geognostic system of Werner is divided into three great classes, the primitive, transition, and floetz rocks,—proposes " to notice particularly the class of rocks named transition," and to show " that they are of an older date than the granite which maintains the first place in point of priority in the system of Werner." How far he has been able to succeed in making out this last position will presently appear.

After telling us that he takes his ideas of the Wernerian geognosy from the third volume of Professor Jameson's Mineralogy (and we allow that he cannot take them from a better source, it being, as he justly states, ".the most authentic account we have of the system taught at Freyberg;") after, I say, telling us this, giving us the definition of grey wacke, and the other rocks constituting the transition class, with a sketch of their geographic distribution in the islands of Great Britain and Ireland, Mr. Allan proceeds to informs us, that he made a tour last summer through Cornwall and Devonshire, and there found that the Wernerian geognosy would not apply. He knew, he says, that granite abounded in the Stannaries, and that tin and wolfram occurred in it; and as these metals (according to his view) never do occur but in the oldest granite, therefore the granite of Cornwall must be the oldest. Here, " therefore, he expected to find a perfect epitome of the Wernerian system, containing the usual series of primitive rocks, descending from granite through gneiss, mica-slate, and clay-slate, with all the *et cæteras* of serpentines, traps, and porphyries; but in this he was mistaken." Certainly he was mistaken, but in a very different way from that in which he conceived himself to be. Let us see what Professor Jameson, in his Elements of Geognosy (the work which Mr. Allan constantly refers to), says with respect to the occurrence of tin and wolfram. In treating of tin, he tells us that it occurs in *very old veins* that traverse granite, gneiss, mica-slate, and clay-slate; that it occurs disseminated through granite, and in *beds* that alternate with that rock. He adds, that the granite appears to belong to the *newest* formation. (El. of Geognosy, p. 261.) At p. 309 of the same work, in the tabular view, the Professor gives us again the geognostic situation of tin, and the only granite mentioned is the newest. Of wolfram, he says, at p. 261, that it occurs in veins both in primitive and transition mountains. And again, at p. 311, in the tabular view, wolfram is stated as occurring not in the oldest, but only in the newest granite formation.

What shall we think, then, of Mr. Allan's accuracy, when he maintains that, according to the Wernerian geognosy, the granite of Cornwall must be referred to the first or oldest formation, because it contains tin and wolfram? But his inaccuracies, or mistatements, do not stop here. He gives it as a Wernerian principle that primitive rocks contain no mechanical deposites: yet Professor Jameson, in his Elements, (the work, as I said before, constantly referred to by Mr. Allan,) pp. 101 and 351, informs us that conglomerates are found in primitive country. Mr. A. asserts, also, that floetz rocks never occur in a conformable position in regard to transition strata. But how he was enabled to ascertain this grand point, he has nowhere condescended to inform us; and yet it is very much to be desired that he had, for no such statement appears either in Professor Jameson's writings, or in any other geognostic work with which I have ever met.

The granite of Cornwall is said to have the same characters as the first or oldest granite of Werner; but it would appear from Mr. A.'s description of the Cornish granite that it contains *fragments*. Now who ever heard of fragments being found in the first granite formation. Professor Jameson, from whose work Mr. Allan assures us he took whatever statements he has made respecting the Wernerian geognosy, distinctly mentions that fragments occur only in the newer formation of granite, that, namely, which borders on the transition rocks. It thus, then, appears from Mr. Allan's own account, that the granite of Cornwall belongs to the newest, not to the oldest granite formation; and that therefore this granite is not, as he asserts, "the nucleus round which Werner conceives all other rocks were deposited."

Mr. Allan tells us that granite veins, according to the Wernerian geognosy, occur only in such rocks as are composed of the same ingredients, such as gneiss and mica-slate. But in none of the books on Werner's system, in as far at least as I have had an opportunity of consulting them, does any such assertion appear. Besides, it is to be observed that mica-slate is not composed of the same ingredients with granite; and Professor Jameson, p. 107, informs us that granite veins traverse clay-slate. Is Mr. A. prepared to contend that this rock is composed of the same ingredients with granite? "The killas of Cornwall," we are told, "belongs to the transition series of Werner;" and this seems to be inferred from its occurring in a country said to be constructed principally of transition rocks. But the killas, Mr. A. observes, assumes the appearance of gneiss whenever it either approaches, or is in immediate contact with, the granite. "It will be observed," says he, "by the specimens from St. Michael's Mount, that the killas there assumes the appearance of fine-grained gneiss." I believe most mineralogists will conclude from this statement, that it really is fine-grained gneiss; and that Mr. Allan's conclusion, that the transition rocks are here connected with granite, is entirely unfounded and fallacious. The granite is probably the second conformable formation of that rock as discovered by Professor Jameson; and I have little doubt that, on farther examination, it will be found that this famous discovery of the metamorphosis of grey-wacke into gneiss, has as little foundation in nature as the metamorphosis of Battus into a touch-stone, or any other of the fine and fanciful changes of bodies into new forms, or if we translate literally, forms into new bodies, to be met with in the writings of Ovid.

Mr. A. infers this change, or passage of grey-wacke into gneiss, from appearances at St. Michael's Mount, and at the Lauran in Galloway. He tells us that at the latter place he took the killas in the vicinity of the granite for mica-slate, "although he had observed no line of separation between it and the killas." Does he mean that the "alteration" produced on grey-wacke, or killas, as he prefers calling it, was actually observed by him to be of "a gradual nature," and "imperceptible" in the neighbourhood of the Lauran, so as to make it small-grained gneiss? or does he only intend to say, that he had no opportunity of seeing the junction of these two rocks? If the former be Mr. Allan's meaning, I wish very much he had mentioned any particular spot where such gradual alteration is to be seen; for though I have been at some pains to discover the junction of what I conceive to be fine-grained gneiss with the grey-wacke of that district, I have not been so fortunate as to meet with any place where the rock is sufficiently exposed for this purpose. But if Mr. A. means merely to affirm that this was the case with him too, and that he had no opportunity of observing the junction of the two rocks of which we are speaking, then certainly his conclusion is such as no mineralogist can admit, and of a very different nature from what might have been expected. That he did not see the distinction, can never be a good reason why he should conclude that there was none.

But admitting, Sir, that Mr. A. did observe a seemingly gradual transition of the grey-wacke into gneiss, is this a proof that these two rocks are one and the same rock? As well might he argue that the colours green and blue in the prismatic spectrum are the same; because the alteration of the green to the blue is of a gradual nature, and imperceptible; or as well might I argue that gneiss and mica-slate are the same, because they are often seen at their junction apparently passing into one another.

Much stress is laid on the miners of Cornwall making no distinction between grey-wacke and the fine-grained gneiss; but including both under the general name of killas. But, to use

Mr. Allan's own words, "the shades of distinction chronicled by the mineralogist cannot be expected to attract the attention of the miner." Mr. Allan would surely not contend that grey-wacke and greenstone are the same because they are often confounded by the vulgar under the name of whinstone. That a writer, who is at such pains to press (and justly) on geologists the "necessity of minute attention" and "accurate examination," as "the only solid basis of philosophic inquiry," should thus, when it seems to suit his own argument, feel so much disposed to pay attention to the mere appellations of the vulgar, is surely rather extraordinary.

> ——— Quid non mortalia pectora cogis,
> Theoriæ sacra fames!

That the rock which lies immediately on the granite of the Lauran is of a very different composition from the grey-wacke in its neighbourhood, is, I think, evident, not only from its qualities as they appear to the eye in hard specimens; and from its superior hardness and toughness under the hammer, as Mr. A. expresses it; but very much from the manner in which it undergoes decomposition when exposed to the air. Any person accustomed to view these two rocks will at once distinguish in the field a piece of the fine-grained gneiss from the grey-wacke rock with which it is accompanied. The grey-wacke disintegrates sparingly, and always nearly uniformly over the whole surface exposed; whereas the fine-grained gneiss disintegrates very much, and always in an irregular manner, showing deep pits and eminences, or ridges and furrowings on the surface. This, there can be little doubt, arises from the felspar contained in its composition, which we know undergoes decomposition very strikingly on many occasions.

I consider myself as now warranted, from what I have said, to draw the following conclusions directly the reverse of those deduced by Mr. Allan.

"The killas of Cornwall," that is, the rock which lies immediately over the granite of that country, "belongs *not* to the transition series of Werner."

"The granite of Cornwall is *not* possessed of every character by which the oldest varieties are distinguished."

"That granite, the nucleus round which Werner conceives all other rocks were deposited, is *not* proved to be in some cases actually of a later date than the transition series."

"Hence the distinction of transition rocks is *not* proved to be grounded on false conclusions."

"And, finally, Werner" need not "make very material alterations in his present system" in order "to accommodate it to the phenomena so commonly presented in nature."

Had Mr. A.'s paper been written in a less assuming tone, I certainly should have perused it with more pleasure than I have done. It is painful to have had to point out so many errors and mistatements in an essay which scarcely contains one fact not previously known to mineralogists, either from the writings of Hutton, of Playfair, of Jameson, or of Berger. When the celebrated Von Buch, in 1806, discovered granite in transition country, he did not immediately infer "that the distinction of transition rocks was thereby proved to be grounded on false conclusions;" he only said, that he had been enabled by this observation to add a new member to the transition series, and thereby increase its importance in the eye of the mineralogist. This was evincing a modesty as well as a sagacity in the great naturalist, which it would have been pleasing to see displayed more conspicuously in the "Remarks on Transition Rocks."

I am, Sir, with much esteem and respect,
Your most obedient servant,
JAMES GRIERSON.

ARTICLE VIII.

Observations on the Daltonian Theory of Definite Proportions.

By Thomas Thomson, M.D. F.R.S.

(*Continued from* p. 52.)

THOUGH the table given in the part of this paper already published be pretty long, it contains but a small portion of the numerous chemical compounds known, and analysed with tolerable accuracy, and therefore capable of being referred to the doctrine of definite proportions. I shall, therefore, in this and some subsequent Numbers, continue it, as far as the present state of chemical analysis will enable us to go. I shall preserve the same method which I followed in the first part of the table; namely, state below, in the form of notes, the documents upon which the numbers adopted in the table are founded.

Sulphurets.

	Number of atoms.	Weight of an integrant particle.
110. Sulphuret of gold	1 *g* + 3 *s*	30·968 [a]

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[a] Gold cannot be united directly with sulphur; yet both from analogy and from the black precipitate which falls when hydrosulphuret of potash is dropt into solution of gold, there can be

	Number of atoms.	Weight of an integrant particle.
111. Sulphuret of platinum	1 *p* + 2 *s*	16·161 [b]
112. Sulphuret of silver	1 *s* + 1 *s*	14·618 [c]
113. Prosulphuret of mercury	1 *m* + 1 *s*	27·000 [d]
114. Persulphuret of mercury or cinnabar	1 *m* + 2 *s*	29·000 [e]
115. Sulphuret of copper	1 *c* + 1 *s*	10·000 [f]
116. Magnetic pyrites	1 *i* + 2 *s*	10·666 [g]

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little doubt that a sulphuret of that metal exists. Now, according to the experiments of Oberkampf, this sulphuret is composed of 100 gold + 24·39 sulphur. Now an atom of gold is 24·968, and 100 : 24·39 :: 24·968 : 6·09. Now 6·09 is almost exactly equal to 3 atoms of sulphur.

[b] According to the experiments of Berzelius, 100 platinum combine with 32·8 of sulphur. (*Lärbok i Kemien*, ii. 424.) Now an atom of platinum weighs 12·161; and 100 : 32·8 :: 12·161 : 3·989; but 3·989 is very nearly equal to the weight of 2 atoms of sulphur.

[c] According to Berzelius, 100 silver combine with 14·9 sulphur. Now 100 : 14·9 :: 12·618 : 1·88; but 1·88 is not much less than the weight of an atom of sulphur; all other analysts make the weight exceed 14·9 a little.

[d] When the salts of mercury containing the black oxide are dissolved in water, and a current of sulphureted hydrogen passed through the solution, a black powder precipitates, composed, according to Sefström's analysis, of 100 mercury + 8·005 sulphur. (*Lärbok i Kemien*, ii. 350.) Now 100 : 8·005 :: 25 : 2·001; but 2·001 is almost exactly equal in weight to an atom of sulphur.

[e] Cinnabar, according to Sefström's analysis, is composed of 100 mercury + 16·01 sulphur. Now 100 : 16·01 :: 25 : 4·002; but 4·002 is almost exactly equal in weight to 2 atoms of sulphur. (*Lärbok i Kemien*, ii. 348.)

[f] From the most exact experiments hitherto made, it appears that 100 copper combine with 26 sulphur. Now 100 : 26 :: 8 : 2·08; but 2·08 differs but little from the weight of an atom of sulphur.

[g] According to the analysis of Proust and Hatchett, magnetic pyrites is composed of 100 iron + 60 metal. Now 100 : 60 :: 6·666 : 3·9996; but 3·9996 is almost exactly the weight of 2 atoms of sulphur. Berzelius found it composed of 100 iron + 58·75 sulphur. This gives 3·916 for the sulphur, still very near the weight of 2 atoms.

	Number of atoms.	Weight of an integrant particle.
117. Cubic pyrites	1 *i* + 4 *s*	14·666 [h]
118. Sulphuret of nickel	1 *n* + 1 *s*	5·623 [i]
119. Prosulphuret of tin	1 *t* + 1 *s*	16·705 [k]
120. Persulphuret of tin or mosaic gold	1 *t* + 2 *s*	18·705
121. Sulphuret of lead	1 *l* + 2 *s*	29·974 [l]
122. Persulphuret of lead	1 *l* + 4 *s*	33·974 [m]
123. Sulphuret of zinc	1 *z* + 1 *s*	6·315 [n]
124. Sulphuret of bismuth	1 *b* + 1 *s*	10·994 [o]

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[h] From Berzelius' experiments (*Ann. de Chim.* lxxviii. 130), it appears that the quantity of sulphur in cubic pyrites, supposing the iron the same, is just double what exists in magnetic pyrites. Hence cubic pyrites must be a compound of 1 atom iron and 4 atoms sulphur.

[i] According to Proust, 100 nickel combine with 48 sulphur. Now 100 : 48 :: 3·623 : 1·739. This, considering the uncertainty respecting the weight of an atom of nickel itself, is not a greater deviation from the weight of an atom of sulphur than might have been expected. If the weight of an atom of nickel given in my table be correct, then 100 nickel ought to combine with 55·2 sulphur.

[k] From the experiments of Mr. John Davy (Phil. Trans. 1812), it appears that the common sulphuret of tin is composed of 55 tin + 15 sulphur. Now 55 : 15 :: 14·705 : 4·015; but 4·015 is almost exactly the weight of 2 atoms of sulphur. From Mr. John Davy's experiments, it appears that mosaic gold contains just double the sulphur in common sulphuret.

[l] From the analysis of Berzelius, we learn that galena is composed of 100 lead + 15·42 sulphur. (*Lärbok i Kemien*, ii. 276.) Now 100 : 15·42 :: 25·974 : 4·005; but 4·005 is almost exactly the weight of 2 atoms of sulphur. The analysis of Mr. John Davy differs very little from that of Berzelius.

[m] Some years ago I received a specimen of galena from Sir John Sinclair, which contained twice the usual quantity of sulphur. Hence it is obvious that a persulphuret of lead exists.

[n] From the experiments of Gueniveau and Kidd, it appears that blende consists of 100 zinc + 49 sulphur. Now 100 : 49 :: 4·315 : 2·114; but 2·114 does not differ much from the weight of an atom of sulphur. As sulphuret of zinc has not yet been made artificially, and as blende contains iron, we cannot expect our analysis to come nearer the truth.

[o] According to Mr. John Davy, 67·5 bismuth unite with 15·08 of sulphur. Now 67·5 :: 15·08 :: 8·994 : 2·009. But

	Number of atoms.	Weight of an integrant particle.
125. Sulphuret of antimony	1 *a* + 2 *s*	15·111 [p]
126. Sulphuret of tellurium	1 *t* + 2 *s*	8·107 [q]
127. Sulphuret of arsenic or realgar	1 *a* + 1 *s*	8·000 [r]
128. Orpiment	1 *a* + 2 *s*	10·000
129. Sulphuret of cobalt	1 *c* + 1 *s*?	9·326? [s]
130. Sulphuret of manganese	1 *m* + 1 *s*	9·130 [t]

2·009 is almost exactly the weight of an atom of sulphur. According to Lagerhjelm, 100 bismuth combine with 22·52 sulphur. Now 100 : 22·52 :: 8·994 : 2·030. This comes also very near the weight of an atom of sulphur.

[p] According to Berzelius, sulphuret of antimony is composed of 100 antimony + 37 sulphur. (*Lärbok i Kemien*, ii. 162.) Now 100 : 37 :: 11·111 : 4·111. According to Mr. John Davy, 42·5 antimony combine with 14·86 sulphur. Now 42·5 : 14·86 :: 11·111 : 3·885. The mean of the two numbers 4·111 and 3·885 is 3·998, which is almost exactly the weight of 2 atoms of sulphur.

[q] According to Davy, tellurium by fusion combines with nearly its own weight of sulphur. Now as an atom of tellurium weighs 4·107, it is clear that the sulphuret must contain 2 atoms of sulphur.

[r] I do not know in what manner Thenard ascertained the composition of the sulphuret of arsenic; but he informs us that realgar is a compound of 3 arsenic + 1 sulphur. Now 3 : 1 :: 6 : 2. Hence it must be a compound of 1 atom arsenic and 1 atom sulphur. Orpiment, he says, is composed of 4 arsenic + 3 sulphur. Now 4 : 3 :: 6 : 4·5. This does not differ very much from the weight of 2 atoms of sulphur. Hence the proportions given in the table. (See *Ann. de Chim.* lix. 284.) Laugier has lately endeavoured to prove that there is only one sulphuret of arsenic, composed of 42 sulphur and 58 arsenic. (*Ann. de Chim.* lxxxv. 26.) If he be correct, it is a compound of 1 atom arsenic with 2 atoms sulphur.

[s] We know but imperfectly the composition of sulphuret of cobalt. Proust says that 100 parts of cobalt absorb 40 of sulphur. (*Ann. de Chim.* lx. 272.) But he puts little confidence in the result of his own experiment. Now 100 : 40 :: 7·326 : 2·9304. As 2·9304 approaches nearest to 1 atom of sulphur, I have supposed the sulphuret a compound of 1 atom cobalt and 1 atom sulphur; but the estimate is entitled to little confidence.

[t] We have no good data for determining the composition of sulphuret of manganese. Vauquelin made a combination of

	Number of atoms.	Weight of an integrant particle.
131. Sulphuret of molybdenum	$1\,m + 2\,s$	9·882[u]
132. Sulphuret of potassium	$1\,p + 1\,s$	7·000[x]
133. Sulphuret of potash	$1\,p + 1\,s$	8·000[y]
134. Sulphuret of sodium	$1\,s + 2\,s$	9·882[z]

We have not the requisite data for determining the composition of the other sulphurets.

A slight inspection of the preceding table is sufficient to convince any person that a very considerable number of sulphurets remain still unknown. Out of 25 sulphurets enumerated, 12 are compounds of 1 atom metal and 1 atom sulphur, 10 of 1 atom metal and 2 atoms sulphur, 1 of 1 atom metal and 3 atoms sulphur, and 2 of 1 atom metal and 4 atoms sulphur. It is not unlikely that silver, copper, nickel, zinc, bismuth, cobalt, manganese, and potassium, may combine only with 1 proportion of sulphur; but gold ought to combine with 3 proportions, and form 3 sulphurets; platinum should form 2, iron 4, lead 4, antimony 2, tellurium 2, and molybdenum 2.

I shall not continue the table any farther at present; but conclude this paper with a few obvious deductions from that part of the table which has been already published.

1. There is no connection between the specific gravity of bodies and the weights of their atoms.

2. There are eight atoms of simple bodies whose weights are denoted by whole numbers; namely,

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74·5 manganese and 25·5 sulphur by heat. (*Ann. du Mus: d'Hist. Nat.* xvii. 16.) Now 74·5 : 25·5 :: 7·130 : 2·345. As 2·345 is not very remote from the weight of an atom of sulphur, I have supposed the sulphuret a compound of 1 atom metal + 1 atom sulphur.

[u] According to Bucholz, sulphuret of molybdenum is composed of 60 molybdenum + 40 sulphur. Now 60 : 40 :: 5·882 : 3·921. But 3·921 is almost equal in weight to 2 atoms of sulphur.

[x] This is the composition of sulphuret of potassium according to Sir H. Davy, the only person who has hitherto analysed this sulphuret.

[y] This compound has never been accurately analysed. I state its composition merely from analogy.

[z] According to Davy.

1. Oxygen	1
2. Sulphur	2
3. Potassium	5
4. Arsenic	6
5. Copper	8
6. Tungsten	8
7. Uranium	12
8. Mercury	25

3. An atom of phosphorus is ten times as heavy as an atom of hydrogen. None of the other atoms appear to be multiples of 0·132; so that if we pitch upon hydrogen for our unit, the weight of all the atoms will be fractional quantities, except that of phosphorus alone.

4. It is quite clear that neither hydrogen nor azote contain any oxygen; for as the weight of an atom of each of these bodies is less than the weight of an atom of oxygen, it is impossible that oxygen can enter into their composition. Hence the whole doctrine respecting ammonium, and the supposition that hydrogen and azote are oxides of ammonium—a supposition countenanced at first by Davy, and still supported by Berzelius, must be erroneous. For the same reason, Berzelius' notion that azote is a compound of oxygen and an unknown combustible gas, cannot be admitted.

5. The weight of an atom of the metals being high, when compared with that of oxygen, or the simple combustibles, they may be, and probably are compounds; though, from their peculiar properties, they may long elude all our methods of decomposition. Their conducting power is too perfect to enable us to hope for any decomposition from the action of the galvanic battery, however powerful.

6. We have it now in our power to compare the law laid down by Berzelius; namely, that the weight of the sulphur in a metallic sulphuret is double that of the oxygen in the oxide of the same metal, with the truth. We shall find it to hold in all the sulphurets hitherto examined, except those of nickel, cobalt, and tellurium. The following table exhibits the particular oxides and sulphurets that have this relation to each other.

Oxides.	Corresponding Sulphurets.
Peroxide of gold	Sulphuret of gold.
Peroxide of platinum	Sulphuret of platinum.
Oxide of silver	Sulphuret of silver.
Protoxide of mercury	Prosulphuret of mercury.
Peroxide of mercury	Persulphuret of mercury.
Protoxide of copper	Sulphuret of copper.

Oxides.	Corresponding Sulphurets.
Deutoxide of iron	Magnetic pyrites.
Deutoxide of tin	Mosaic gold.
Deutoxide of lead	Common galena.
Peroxide of lead	Persulphuret of lead.
Oxide of zinc	Sulphuret of zinc.
Oxide of bismuth	Sulphuret of bismuth.
Deutoxide of antimony	Sulphuret of antimony.
Deutoxide of arsenic	Orpiment.
Protoxide of manganese	Sulphuret of manganese.
Deutoxide of molybdenum	Sulphuret of molybdenum.
Potash	Sulphuret of potassium.
Soda	Sulphuret of sodium.

(To be continued.)

ARTICLE IX.

Remarks on the Measurement of Minute Particles, especially those of the Blood and of Pus. From Dr. Young's Medical Literature, 8vo. Lond. 1813, p. 546.

I. *On the Form and Magnitude of the Particles of the Blood.*

THE form and magnitude of the coloured particles of the blood is a subject not only interesting and important in itself, but is also capable of assisting, by means of comparative observations, in the determination of the magnitude of the capillary arteries, and the investigation of the resistance which they exhibit; it may also be of advantage to obtain some tests capable of ascertaining, whether these particles undergo any change in diseases of various kinds, and what is their relation to the globules of pus, and of other animal fluids: hitherto the measures of the particles of blood, which have been considered by various authors as the most accurate, have differed no less than in the ratio of 2 to 5; and there is an equal degree of uncertainty respecting their form, some admitting the truth of Mr. Hewson's opinions, and a greater number rejecting them without any satisfactory evidence. In such examinations, it is only necessary to employ a full and unlimited light, in order to obtain a very distinct outline of what appears manifestly to be a very simple substance, and we thus seem to have the clear evidence of the senses against Mr. Hewson: but we must remember, that where the substances to be examined are perfectly transparent, it is only in a confined and diversified light that we can gain a correct idea of their structure. The eye is best prepared for the investigation, by beginning with the blood of a skate, of which the particles are so conspicuous, and of so unequivocal a form, as to set aside at once the idea of a simple homogeneous substance. They are oval and depressed, like an almond, but less pointed, and a little flatter; each of them contains a round nucleus, which is wholly independent in its appearance of the figure of the whole disc, being sometimes a little irregular in its form; seldom deviating from its central situation, but often remaining distinctly visible while the oval part is scarcely perceptible; and as the portion of blood dries away, becoming evidently prominent above the thinner portion. This nucleus is about the size of a whole particle of the human blood, the whole oval being about twice as wide, and not quite three times as long; the nucleus is very transparent, and forms a distinct image of any large object which intercepts a part of the light by which it is seen, but exhibits no inequalities of light and shade, that could lead to any mistake respecting its form. But if we place some particles of human blood under similar circumstances, near the confine of light and shade, although they are little, if at all, less transparent, we immediately see an annular shade on the disc, which is most marked on the side of the centre on which the marginal part appears the brightest, and consequently indicates a depression in the centre, which Delatorre mistook for a perforation. It is most observable when the drop is drying away, so that the particles rest on the glass: and when a smaller particle is viewed, it has merely a dark central spot, without any lighter central space. Nor have the particles ever appeared to me "as flat as a guinea," although their axis is sometimes not more than one-third or one-fourth of their greatest diameter; if they were much thinner than this, their diameter would be more diminished than it is when they become spherical, by the effect of an aqueous fluid: while this form corresponds to a diminution to about $\frac{2}{3}$ of the original diameter. They may indeed possibly absorb a part of the surrounding moisture in the change: but they do not seem to have their dimensions much affected by the fluid in which they are suspended, since they may easily be spread thin on glass, and dried, without much change of their magnitude, at least in the direction of the surface to which they adhere: and they remain distinct as long as the access of moist air is completely excluded. When they have been kept for some time in water, and a little solution of salt is added, their form and structure, as Mr. Hewson has observed, are more easily examined, and appear to resemble those of a soft substance with a denser nucleus, not altogether unlike the crystalline lens together with the vitreous humour, as seen from behind: but with respect to a central particle detached within a vesicle, "like a pea in a bladder," I cannot doubt that Mr. Hewson was completely mistaken. I have never observed a prominence in the outline of the particles of the human blood: and on the other hand I am not perfectly confident that the apparent depression, which is exhibited in some lights, may not depend on some internal variation of the refractive density of the particle. It has commonly been asserted, that these coloured particles are readily soluble in water; but this opinion appears to be completely erroneous, and to depend partly on their passing readily through filtering paper, a circumstance indeed already observed by Berzelius, (Djurk. ii. p. 3,) and partly on the extraction of a great part of their colouring matter, together with which they lose much of their specific gravity, so that instead of subsiding, they are generally suspended in the fluid; their presence may, however, still be detected by a careful examination, and they seem in this state to have recovered in some measure their original form, which they had lost when first immersed in the water. When the water is sufficiently diluted, about three-fourths as much rectified spirits may be added to it without destroying the appearance; but after a few months it becomes indistinct, although neither in this case nor in that of complete putrefaction do the globules appear to become constituent parts of a homogeneous fluid. The existence of solid particles, in fluids which at first sight appear transparent, is the most easily detected, by looking through them at a small luminous object, either directly or by reflection, as, for example, at the image of a candle seen at the edge of a portion of the fluid, held in a tea-spoon; in this case, wherever there are small particles in suspension, for instance, in milk diluted with water, they will produce a minutely tremulous or sparkling appearance, which is rendered still more distinct by the assistance of a lens, and which depends on the diversified interception of the light, while the particles are carried over each other by the internal motion of the fluid. This test is applicable to all cases of minute particles held in suspension; where however the greater number of the particles are nearly equal in dimensions, the luminous object viewed through them exhibits a much more striking appearance, for it is surrounded by rings of colours, somewhat resembling those of the rainbow, but differently arranged, and often beautifully brilliant. The blood, a little diluted, always exhibits them in great perfection, and they afford a very accurate criterion for the distinction between pus and mucus: mucus, containing no globules, affords no colours, while those which are exhibited by pus exactly resemble the appearance produced by the blood, the rings being usually of the same dimensions: whence it follows that the globules are also of the same size, for the dimensions of the rings vary with those of the particles which produce them: and there can be little doubt, from this circumstance, that the globules found in pus are the identical globules of the blood, although probably somewhat altered in the process of suppuration. A minute quantity of the fluid to be examined in this manner may be put between two small pieces of plate glass, and if we hold the glass close to the eye, and look through it at a distant candle, with a dark object behind it, the appearance, if the globules are present, will be so conspicuous as to leave no doubt respecting their existence.

II. *Description of an Eriometer.*

The rings of colours, which are here employed to discover the existence of a number of equal particles, may also be employed for measuring the comparative and the real dimensions of these particles, or of any pulverised or fibrous substances, which are sufficiently uniform in their diameters. Immediately about the luminous object, we see a light area, terminating in a reddish dark margin, then a ring of bluish green, and without it a ring of red: and the alternations of green and red are often repeated several times, where the particles or fibres are sufficiently uniform. I observed some years ago that these rings were the larger as the particles or fibres affording them were smaller, but that they were always of the same magnitude for the same particles. It is therefore only necessary to measure the angular magnitude of these rings, or of any one of them, in order to identify the size of the particles which afford them; and having once established a scale, from an examination of a sufficient number of substances of known dimensions, we may thus determine the actual magnitude of any other substances which exhibit the colours. The limit between the first green ring, and the red which surrounds it, affords the best standard of comparison, and its angular distance may be identified by projecting the rings on a dark surface, pierced with a circle of very minute holes, which is made to coincide with the limit, by properly adjusting the distance of the dark substance, and then this distance, measured in semidiameters of the circle of points, gives the corresponding number of the comparative scale. Such an instrument I have called an Eriometer, from its utility in measuring the fibres of wool, and I have given directions for making it, to Mr. Fidler, in Foley-street. The luminous point is afforded by a perforation of a brass plate, which is surrounded by the circle of minute holes; the substance to be examined is fixed on some wires, which are carried by a slider, the plate being held before an Argand lamp, or before two or three candles placed in a line; the slider is drawn out to such a distance as to exhibit the required coincidence, and the index then shows the number representing the magnitude of the substance examined. The instrument may be rendered more portable, though somewhat less accurate, by merely making the perforations in a blackened card, furnished with a graduated piece of tape. An eye not shortsighted will generally require the assistance of a lens, when the instrument is made of the most convenient dimensions, which I have found to be such as to have two circles of points, one at $\frac{1}{3}$ and the other $\frac{1}{5}$ of an inch in semidiameter, with their corresponding scales. The central perforations are about $\frac{1}{30}$ and $\frac{1}{50}$ of an inch in diameter; the points 8 or 10 only in each circle, and as minute as possible. The light of the sun might also be employed, by fixing the circle of points at the end of the tube of a telescope: but it rather adds glare than distinctness to the colours: nor have I been able to gain any thing by looking through coloured glasses, or by using lights of different qualities. Where the object consists of fibres which can be arranged in parallel directions, a fine slit in the plate or card affords brighter colours than a simple perforation, and the points must in this case be arranged in lines parallel to the slit; but if care is not taken to stretch the fibres sufficiently, the employment of the slit in this manner will make them appear coarser than they really are. The colours will still appear, even if there be a considerable difference in the dimensions of the fibres or particles, but they will be so much the less distinct as the difference is greater. In this case the measure indicated will be intermediate between the extreme dimensions; although most commonly it will be seen somewhat below the true mean, the colours exhibited by the finer fibres prevailing in some degree over the rest. The latitude, however, which the eriometer affords in the regularity of the substances measured by it, and its collecting into one result the effect of many thousands of particles, or of an endless variety of small differences in the diameters of fibres, give it an unquestionable preference over every kind of micrometer which measures a single interval only at once, with respect to all applications to agriculture or manufactures; for in reality there is not a single fibre of wool among the millions which constitute a fleece, that preserves an uniform diameter throughout its length, and the difference is still greater between the fibres which grow on different parts of the animal; so that to take a single measurement, or even any practical number of measurements, by the most accurate micrometer, in the usual acceptation of the term, for a criterion of the quality of a fleece, can tend only to the propagation of error or conjecture in the semblance of the minutest accuracy. Even with the eriometer, the difficulty of obtaining a fair average of the quality of a sample of wool is extremely great; it is absolutely necessary to preserve the fibres as much as possible in their natural relative situation, and to examine them near the middle of their length; the ends next the skin are almost always considerably finer, and the outer ends generally coarser, than the rest; but this difference is greater in some kinds of sheep than in others, and as far as I have observed, it is less in the Merinos and their crosses than in other sheep: there is also far less difference in the different parts of the same fleece in these breeds than in others; still however this difference is very observable, although it is probable that some part of the sheep might be found, which in all cases might fairly be considered as affording nearly the average of the whole fleece; and I imagine that the part of the back about the loins is the most likely to be possessed of this property; so that the middle of the fibres of this part of the fleece might be assumed, in the finer kinds of wool, as affording a fair measure for the whole.

III. *Scale of the Eriometer.*

The theory, which suggested to me the construction of the eriometer requires some corrections in its immediate application, which depend on circumstances not completely understood: at present, therefore, I shall only employ, for the determination of the true value of the numbers of its scale, an experimental comparison of its indications with some microscopical measurements, which Dr. Wollaston has been so good as to perform for me, with an admirably accurate micrometer of his own invention.

The dust or seed of the lycoperdon bovista he finds to be $\frac{1}{8500}$ of an inch in diameter: this substance gives very distinctly 3·5 on the scale of the eriometer; and 3·5 × 8500 = 29750. The globules of the blood measured $\frac{1}{4900}$; and immediately afterwards, when examined in the same state by the eriometer, indicated about 6½; and 6·5 × 4900 = 31850. A wire of platina, obtained by a very ingenious method, peculiar to Dr. Wollaston, measured $\frac{1}{3200}$; and when coiled up, gave n. 9 of the eriometer; and 9 × 3200 = 28800. The mean of a considerable number of comparative observations on fibres of wool, between n. 20 and 30, afforded also 28800 for a product.

A mean of these experiments gives very nearly $\frac{1}{30000}$ for the unit of the scale of the eriometer. Some former investigations had led me to attribute to this unit a value somewhat smaller, especially for the lowest numbers; and I had obtained a formula, and made a table, for ascertaining the true dimensions of any substance measured by the instrument, according to the result of these investigations: but since my later experiments seem to have superseded the mode of calculation which I had adopted, I think it unnecessary to insert the table.

Having sufficiently ascertained the true value of the indications of the eriometrical scale, I shall now enumerate the measurements of the principal substances which I have examined with the instrument.

IV. *Substances measured by the Eriometer.*

Milk, diluted, very indistinct, about	3
Dust of lycoperdon bovista, very distinct	3½
Bullock's blood, from beef	4½
Smut of barley, called male ear	6⅓
Blood of a mouse	6½
Human blood diluted with water, 5; after standing some days 6, or	7
Blood recently diluted with serum only	8
Pus	7½
Silk, very irregular, about	12
Beaver wool, very even, (jointed)	13
Angola wool, about	14
Vigonia wool	15
Siberian hare's wool, Scotch hare's wool, foreign coney wool, yellow rabbit's wool, about	15½
Mole's fur, about	16
Skate's blood, very indistinct, about	16
American rabbit's wool, British coney wool, about	16½
Buffalo's wool	18
Wool of the ovis montana	18
Finest seal wool, mixed, about	18½
Shawl wool 18, or	19
Goat's wool	19
Cotton, very unequal, about	19
Peruvian wool, mixed, the finest locks	20
A small lock of Welsh wool	20
Saxon wool, a few fibres 17, some 23, chiefly	22
An Escurial ram, at Lord Somerville's show, 23 to	24
Mr. Western's South Down, some specimens	24½
Lioneza wool, 24 to 29, generally	25
Paular wool, 24 to 29, generally	25½
Alpacca wool, about	26
Farina of laurestinus	26
Ryeland Merino wool, Mr. Henty	27
Merino South Down wool, Mr. Henty	28
Seed of lycopodium, beautifully distinct	32
South Down ewe, Mr. W. B.	39
Coarse wool, Sussex	46
Coarse wool, from some worsted	60

It would not be difficult to obtain from these measures a tolerable approximation to the value of wool at its usual prices. If we square the number, and subtract 325, the remainder will be about the number of pounds that are worth 100 guineas. Thus, for good Lioneza, n. 25, 25 × 25 − 325 = 300, giving 7s. a pound; for moderate South Down, n. 35, 35 × 35 − 300 = 900, or 2s. 4d. a pound: which is probably about the proportional value, though both the proportional and the real values must fluctuate according to the demand of the manufacturer.

(To be continued.)

ARTICLE X.

On Sir H. Davy's Theory of Chlorine, and its Compounds. By Mr. William Henderson, Member of the Royal Medical Society of Edinburgh.

(Continued from p. 13.)

II. *Oxymuriatic Gas, by its Action on Metals and Inflammables, produces Substances differing essentially from the Oxydes of the same Bodies.*

The new theory was, until very lately, secure from assault on this quarter; for except two passages in the Philosophical Transactions, nothing definite had been advanced respecting this point by its supporters. These passages I insert:—

"Muriatic acid gas, as I have shown, and as is farther proved by the researches of MM. Gay-Lussac and Thenard, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decompounded, unless a new combination is formed; thus, it is not changed by charcoal ignited in the gas by voltaic electricity; but it is decompounded by all the metals; and in these cases hydrogen is elicited, in a manner similar to that in which one metal is precipitated by another, the oxygen being found in the new compound."*

"In some experiments, made very carefully by my brother, Mr. John Davy, on the decomposition of muriatic acid gas by heated tin and zinc, hydrogen equal to about half its volume was disengaged, and metallic muriates, the same as those produced by the combustion of tin and zinc in oxymuriatic gas, resulted."†

These two authorities are in very positive terms; and if they are to be relied on, the products of the action of oxymuriatic gas on metals, though not similar to those which result from the action of oxygen on the same bodies, are precisely what they were thought to be, viz. compounds of the metals and oxygen, with which the muriatic acid, condensed from the want of water, has united to form muriates. To this conclusion it will of course be objected, that these passages were written previous to the adoption of the new theory. This objection will, however, vanish, on considering, that it is not to the opinions, but to the facts contained in the passages cited, that my reasoning has reference.

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* Phil. Trans. 1810, p. 67.

† Phil. Trans. 1810, p. 237.

By the publication of Mr. J. Davy's paper on the combinations of chlorine with the metals,* I am enabled to bring this part of the discussion more completely under examination than could before be done; and in doing this I shall be as brief as the nature of the reasoning employed will allow.

The first compound noticed in this paper is cuprane; which is formed "by exposing slips of copper, partially immersed in muriatic acid, to the atmosphere." Mr. J. Davy says that in this process the deliquescent muriate is formed—"which flowing into the muriatic acid is changed by the action of the immersed copper into cuprane." According to this view, cuprane cannot contain more, but may have less chlorine than can be yielded by the muriatic acid of the deliquescent muriate; yet the analysis given ascribes to it a proportion of chlorine exceeding by 12·18 per cent. what it can contain consistently with Chenevix's analysis of the proto-muriate, and approaching within a minute quantity to the proportion given by Proust,† as belonging to the per-muriate.

Again, cuprane is stated to be composed of 64· of copper, and 36· of chlorine. According to Chenevix, the proto-muriate contains 70·25 of protoxyde (of which 63·161 is copper), and 24·75 of muriatic acid. Now it is worthy of notice, that, on the principles of the old theory, the quantity of oxygen contained in the protoxyde, is capable of forming oxymuriatic gas with 24·383 of muriatic acid; so that the only difficulty in reconciling these analyses is the water contained in the proto-muriate, which Mr. J. Davy seems to have laid to the account of the chlorine.

From the analysis of cupranea having been performed on its solution, it appears that Mr. J. Davy in fact examined the deliquescent muriate, into which, he says, that it is converted by the action of water. But the result of Mr. J. Davy's analysis exceeds in the proportion of the chlorine that given by Proust by 26·816 per cent. How can this quantity of chlorine escape from its combination when the compound is dissolved in water, and again enter into union with the copper on the water being separated. Another difficulty also lies against the analysis of cupranea. Mr. J. Davy says that it is composed of 47 of copper and 53 of chlorine. Now 47 of copper, in passing to the state of peroxyde, unite with 15·67 of oxygen; which quantity of oxygen, on the principles of the old theory, can convert 53·93 of muriatic acid into oxymuriatic gas; or (which is the same thing), on the basis of the new theory, can unite with the hydrogen contained in that quantity of muriatic acid gas. But it appears by Proust's analysis, that 62·67 of peroxyde of copper cannot combine with, or act on more than 37·8 of muriatic acid.

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* Phil. Trans. 1812, p. 169.

† Ann. de Chim. tome xxxii. 47.

A deficiency of previous analyses, in which I could confide, prevented my examining in detail Mr. J. Davy's account of the compounds of chlorine with tin, iron, manganese, zinc, arsenic, antimony, and bismuth. I may, however, be permitted to say a few words respecting the compounds of iron. It is stated by Mr. J. Davy, that ferranea has more chlorine than ferrane, and that the solutions of these compounds are the red and green muriates of iron respectively. Sir H. Davy* states, that a solution of the red muriate may be converted into the green by sulphureted hydrogen. This is easily explained, if we suppose, according to the old theory, that the iron is converted into a protoxide by the sulphureted hydrogen: but if we suppose the iron to be in both cases in the metallic state, or, in other words, exposed to a substance, with which it cannot unite until it has assumed that state, the effect of the sulphureted hydrogen seems by no means easily explained.

Horn-lead is said by Mr. J. Davy to contain 74·22 of lead, and 25·78 of chlorine. According to Kirwan's analysis, it has 76 of lead,† and 18·23 of muriatic acid; which, by the new theory, can yield only 17·6 of chlorine. In this case, the proportion of lead seems to answer very well; but that of chlorine is over-rated, by 8·18 per cent.

Again, by the analysis of Kirwan, horn-lead contains 5·77 of oxygen; which, on the principles of the old theory, are sufficient to form 25·64 of oxymuriatic gas, by union with muriatic acid. Is this very singular coincidence, with the others before stated, to be regarded as merely accidental?

III. *When carefully dried, Oxymuriatic Gas is incapable of acting on Charcoal.*

This seems accounted for, on the old theory, by saying, that the oxymuriatic gas, if it acted on the charcoal, and imparted its oxygen to it, must give rise to a quantity of muriatic acid; but that this acid being incapable of existing in an insulated state, and there being no water present, those actions by which its evolutions would be caused are prevented. In this way, the whole comes to be merely an additional instance of the power of that modification of affinity which has been named disposing or resulting. According to the new theory, on the other hand, this want of action admits of no explanation or reference to any general class of phenomena; but must be ranked as an ultimate and inexplicable fact.

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* Researches, &c. p. 182.

† Thomson's System, &c. iii. p. 265.

In some instances, as in that of hydrogen and nitrogen, the nascent state favours combination. In others, as corundum with acids, the absence of cohesion is necessary. In a third class, as nitrogen and oxygen, a previous partial union effects a combination, otherwise nearly impracticable. A case such as the present requires the most scrupulous attention to all these circumstances, as far as is possible. The last of these has been made the subject of experiment by Mr. Murray,* and by Mr. J. Davy,† who, though he first asserted that dried carbonic oxide was not acted on by oxymuriatic gas, states in a subsequent paper,‡ that they combine on simple exposure to day-light; and that from the combination there results a peculiar acid gas, composed of chlorine and carbonic oxide. May it not be a chemical compound of muriatic and carbonic acids?

Mr. Murray tried some experiments, with a very ingenious arrangement, of oxymuriatic gas, carbonic oxide, and ammoniacal gas. The public are already in possession of the results; but it must be observed, that the confidence to which they might otherwise appear entitled, seems somewhat lessened by the recent discovery of an explosive compound, apparently of oxymuriatic gas and nitrogen.§

IV. *Water is formed in almost all Cases of the Evolution of Oxymuriatic Gas.*

The principal methods of obtaining oxymuriatic gas may be ranked under two heads; viz. 1. Muriatic acid made to act on the peroxide of manganese. 2. Hyperoxymuriate of potash exposed to the action of strong liquid muriatic acid.

1. *Of the Action of Muriatic Acid on the Peroxide of Manganese.*

The common rationale given of this action, previous to the new theory, was, that the peroxide of manganese being insoluble in muriatic acid, while the protoxide yields readily to it, the various existing attractions, aided by heat, suffice to disunite the manganese and the oxygen, which is instantly seized on by the muriatic acid to form oxymuriatic gas. According to Sir H. Davy's ideas of the nature of chlorine, we must have recourse to the attractions between the chlorine and the manganese, and between the hydrogen and the oxygen; which, together with the disposition of the chlorine to assume the elastic form, must be conceived sufficient to overcome the attractions between the manganese and the oxygen, and between the chlorine and the hydrogen. To this explanation, however, besides the gratuitous assumption that chlorine attracts the manganese more strongly than oxygen does, it is absolutely indispensable that hydrogen should attract oxygen more strongly than it attracts chlorine: yet, as is remarked by an anonymous correspondent of Mr. Nicholson,* Sir H. Davy afterwards infers that the attraction between chlorine and hydrogen is the more powerful.†

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* Nicholson's Journal, vol. xxviii. p. 132, &c.

† Nicholson's Journal, vol. xxviii. p. 193, &c.

‡ Nicholson's Journal, vol. xxx. p. 28.

§ See Nicholson's Journal, vol. xxxiv. pp. 180, 270.

2. *Of the Action of strong liquid Muriatic Acid on Hyperoxymuriate of Potash.*

It may be well to take a brief view of the formation of this salt, before proceeding to consider the process for obtaining oxymuriatic gas from it.

If a stream of oxymuriatic gas be passed through a solution of potash, it undergoes a change; in consequence of which we find in the liquid the two salts known by the names of muriate and hyperoxymuriate of potash. According to the old theory, this change consists in the extrication of oxygen from part of the oxymuriatic gas, and the absorption of this oxygen by the remaining portion. On Sir H. Davy's principles, the most direct explanation is, that part of the chlorine decomposes a portion of the water, forming muriatic acid by union with its hydrogen, while the rest of the gas, uniting with the oxygen of this portion of water, gives rise to hyperoxymuriatic, or, as it would almost require to be called, chloric acid. To this explanation it were needless to urge any objection, as Sir H. Davy does not give it his sanction. He holds the salt known by the name of hyperoxymuriate of potash to be composed of a gas, which he has called euchlorine, united to peroxide of potassium. The merits of this opinion I shall have occasion to discuss in the sequel.

Another idea has been started with regard to the nature of this salt by Mr. Crane,‡ who holds it to be composed of chlorine, water, and the unknown base of the alkali. If, however, water be the source of all the oxygen which may be procured by heat from the salt in question, that compound must exist in it in the proportion of at least 44·883 grs. to 100 grs.; and the hydrogen of this (about 6·405 grs.) is sufficient to combine with not less than 226·64 cubic inches of chlorine; while according to the analysis of Chenevix, 100 grs. of hyperoxymuriate of potash cannot possibly contain more than 62·298 cubic inches: so that we should expect an evolution of as much hydrogen as can combine with 164·342 inches of chlorine; yet no admixture of hydrogen has been detected in the oxygen procured by heating hyperoxymuriate of potash. The formation of the muriate along with the hyperoxymuriate is accounted for by Mr. Crane, in supposing part of the muriatic acid to be volatilized by the heat employed: but this must appear insufficient when we reflect, that even if the oxymuriatic gas be passed through a receiver containing water, previous to being brought to act on the alkali, there is a proportion of muriate formed.

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* Nicholson's Journal, vol. xxviii. p. 369.

† Phil. Trans. 1811, p. 158. See also p. 29 of the same vol. where these opposite modifications of affinity are successively reasoned on in two successive paragraphs.

‡ Nicholson's Journal, vol. xxix. p. 44, &c.

In accounting for the decomposition of the liquor of Libavius by ammonia and water, Mr. Crane supposes the chlorine to be attracted from the tin by the ammonia; but Sir H. Davy says,* "Muriate of ammonia, and muriate of magnesia, are perfectly correct expressions;" and, on another occasion,† "Muriate of ammonia is composed of muriatic acid gas and ammonia:" nor have we any evidence that ammonia is capable of direct combination with oxymuriatic gas.

When hyperoxymuriate of potash is acted on by muriatic acid very much diluted, a peculiar gas is disengaged, to which Sir H. Davy gave the name of euchlorine.‡

On applying a very gentle heat to this gas, it explodes, and is resolved into chlorine, and oxygen, its volume being at the same time increased, according to Sir H. Davy, in the proportion of 120 to 100.

This gas is stated by its discoverer to consist of 1 of oxygen united to 2 of chlorine: it is incapable of supporting combustion; and this circumstance is looked on by Sir H. Davy as an argument in favour of his theory. "If," says he, "the power of bodies to burn in oxymuriatic gas depended upon the presence of oxygen, they all ought to burn with much more energy in the new compound."§ This argument is, however, entirely turned aside by what he adds almost immediately after; viz. "Supposing oxygen and oxymuriatic gas to belong to the same class of bodies, the attraction between them might be conceived very weak, as it is found to be." Now it is certainly not easy to conceive that two bodies, both of which are excellent supporters of combustion, and which possess for each other an attraction so feeble as to be easily subverted by the heat of the hand, should, in consequence of so slight a combination, be rendered incapable of performing this office. This supposition seems quite as difficult to be maintained as the one which is necessary to reconcile the fact to the old theory; and which has been laid hold of as an argument against that explanation, viz. that "this muriatic acid with one dose of oxygen supports combustion, and with an additional dose becomes a non-supporter." Improbable as this supposition seems at first sight, we are furnished with a nearly analogous instance in the actions of nitric and nitrous oxides on living animals. Besides, owing to the great facility with which this gas is decomposed, the heat to which the substances experimented on could be exposed must have been very trifling, and far below what is necessary to cause their inflammation in pure oxygen.

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* Phil. Trans. 1811, p. 33.

† Phil. Trans. 1810, p. 239.

‡ Phil. Trans. 1811, p. 155.

§ Phil. Trans. 1811, p. 160.

We are now prepared to examine Sir H. Davy's theory of hyperoxymuriate of potash, which he holds to be composed of euchlorine united to peroxide of potassium. We are not as yet in possession of any very decisive analysis of this peroxide; but Sir H. Davy mentions, that with 2 grains of potassium he obtained an absorption of 2 6 cubic inches of oxygen, the weight of which quantity is ·884 of a grain: while it appears probable that 2 grains of potassium, in passing to the state of potash, absorb only ·278 of a grain, being rather less than one-third of the quantity taken to form the peroxide. According to Chenevix, 100 grs. of hyperoxymuriate of potash contain 39·2 grs. of potash. Now in passing to the state of peroxide, this quantity will absorb 21·98116 grs. of oxygen: but since from 100 grs. of the salt 38·478 grs. of oxygen may be obtained, there remain 16·49684 grs. which must enter into combination with the chlorine to form euchlorine. Sir H. Davy says, as was already remarked, that this gas consists of 2 of chlorine united to 1 of oxygen. If by volume, their relative weights must be as 725 to 170; and in this proportion, 16·49684 grs. of oxygen require 69·86616 grs. of chlorine, a quantity exceeding by 44·05971 grs. the weight of what the salt may be conceived to contain. If Sir H. Davy means, that 2 by weight of chlorine unite with 1 by weight of oxygen (which, from his adducing it in illustration and corroboration of the theory of definite volumes in gases, I cannot think he does), the weight of chlorine required is 32·99368 grains, which still considerably exceeds 25·80645 grains (the quantity contained in 100 grs. of the salt), and which differs yet more from 19·263 grs. the proportional quantity as ascertained by calculation on Sir H. Davy's principles from the quantity of muriate of potash formed along with the hyperoxymuriate.

According to the old theory, 100 grs. of potash, with an much oxymuriatic gas as they can act on, ought to yield nearly 176·456 grs. of saline matter, consisting of 113·176 grs. of muriate, and 63·28 grs. of hyperoxymuriate.

According to Sir H. Davy's theory, the resulting quantity of saline matter ought to be 153·188 grs.; of which 117·535 grs. should be muriate, and 35·653 grs. hyperoxymuriate.[*] He has, however, given a calculation somewhat different from this;[†] by which it appears that 100 grs. of potash yield 168·372 grs. of saline matter, containing 127·4 grs. of muriate, and 40·972 of hyperoxymuriate.

It is only when strong muriatic acid is made to act on the salt that oxymuriatic gas is obtained. It were needless to extend a paper, already too long, by detailing the opposite theories of this process; it may be sufficient to remark, that, according to Sir H. Davy's view, hydrogen must attract oxygen more powerfully than it attracts chlorine.

Let us now examine the phenomena which present themselves during the decomposition of oxymuriatic gas; which readily takes place, if water impregnated with it be exposed to the sun's light. According to the old theory, the light merely separates the oxygen from the muriatic acid. On the principles laid down by Sir H. Davy, the chlorine decomposes the water, uniting with its hydrogen, and setting the oxygen at liberty: so that here the attraction between chlorine and hydrogen must overbalance that between hydrogen and oxygen.

According to the former of these theories, the oxygen comes entirely from the oxymuriatic gas, and a quantity of muriatic acid is left, equal in weight to the gas used, *minus* the weight of the oxygen obtained. If, on the other hand, Sir H. Davy's theory be correct, the oxygen is derived from the water alone, and the residual muriatic acid will be equal to the weight of the chlorine used, *plus* the weight of a quantity of hydrogen, sufficient to form water with the oxygen given out; while the water must be diminished by a quantity equal to the weight of the oxygen obtained, added to the increase in weight on the part of the chlorine.

I had an opportunity, during the summer of 1811, through the kindness of Mr. Garden, of Compton-street, Soho, of performing a few experiments on this point; not, however, with a view to ascertain whether or not the water suffered any diminution, but to determine the quantity of residual muriatic acid, which I estimated by the formation of muriate of silver, founding my calculations on the analysis of that salt given by Proust.* 7·5 cubic inches, or 5·4375 grs. of the gas were agitated with distilled water, until it had absorbed the whole. Nitrate of silver was then added, and, after being several times agitated, the phial was set aside in a dark place for 24 hours. The mixture of muriate of silver and acid liquor was then thrown on a filtre, previously dried with care, and weighed, and the residuum was washed with distilled water, until it passed through tasteless. The filtre with the muriate of silver was next wrapped up, and laid in a dark place, having a temperature of about 70°, until it appeared perfectly dry. It was then found to have gained between 22·5 and 23 grains.

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[*] In this calculation, I have entirely disregarded the uncertainty respecting the constitution of the peroxide of potassium, and of euchlorine; and have proceeded solely on the ground that 100 of the salt give off by heat 38·478 of oxygen.

[†] Phil. Trans. 1811, p. 19.

According to the old theory, 5·4375 grs. of oxymuriatic gas should be resolved into 1·323467 gr. of oxygen, and 4·114033 grs. of muriatic acid, which can combine with 19·28073 grs. of oxide of silver, forming 23·394763 grs. of muriate.

In consonance with Sir H. Davy's views, the chlorine and the silver combine, while the oxygen previously in union with the silver is set at liberty. Now 5·4375 grs. of chlorine are capable of forming, by union with hydrogen, 5·64645 grs. of muriatic acid, which can in their turn produce 31·37917 grs. of horn silver; therefore 5·4375 grs. of chlorine are capable of forming 31·37917 grs. of horn silver.

To this experiment it may be objected, that, if Sir H. Davy's theory be correct, the analysis of muriate of silver assumed must be incorrect in regard to proportions. This objection it will be necessary to examine somewhat minutely. The proportions of acid assigned to the salt by the analyses of Gay-Lussac and Thenard,† Berzelius,‡ Rose, Proust, Zaboada, Chenevix, and Kirwan,§ do not differ from each other more than between 19·44 and 16·54. Of these I have in this paper preferred that of Proust, which is very nearly the mean of them. If, then, 100 grs. of muriate of silver yield, on analysis, 18 grs. of muriatic acid and 82 grs. of oxide of silver, it seems probable that it contains 17·323 grs. of chlorine, united to 75·6 grs. of silver. But this supposition leaves a deficiency of 7·077 grs.; and to fill up this we must suppose the salt to contain a proportion of water, which, being decomposed during its analysis, yields its oxygen to the silver, and its hydrogen to the chlorine. Now 75·6 grs. of silver take 6·4 grs. of oxygen, which can combine with 1·07 gr. of hydrogen, while 17·323 of chlorine cannot unite with more than ·677 of a grain to form muriatic acid; so that an evolution of hydrogen must take place to the extent of ·393 of a gr. or 15·04 cubic inches. Should it be preferred to suppose horn silver composed solely of chlorine and silver, it must yield, on analysis, 20·02 of muriatic acid, and 86·648 of oxide of silver.

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* Journal de Phys. xlix. p. 221.

† Rech. Phys. Chem. ii. 123.

‡ Ann. de Chim. lxxix. 133; lxxviii. 114; lxxvii. 84.

§ See Dr. Thomson's System of Chemistry, iii. 153.

Mr. J. Davy has, in his paper on the combinations of chlorine, stated another analysis of horn silver, which, he observes, agrees very nearly with that given by Klaproth, viz. 24·5 of chlorine and 75·5 of silver. On this basis, the result of the analysis should be 25·4224 of muriatic acid, and 82·4 of oxide of silver. It must, however, be allowed, that if this difficulty, and some others that will be stated immediately as attaching to Mr. J. Davy's analysis, can be got over, it will answer very nearly to the result of the above detailed experiment; for 5·4375 grs. of chlorine ought, on this estimate, to form 22·194 grs. of horn silver.

If oxide of silver, at the precise degree of oxidation in which it has been stated to be found in muriate of silver (viz. containing about 7·8049 per cent. of oxygen) be added to common muriatic acid, there ought to ensue, on Sir H. Davy's theory (if Proust's analysis be correct), an evolution of oxygen to the amount of 7·188 cubic inches for each 100 grs. of the salt formed, because the hydrogen previously united to the chlorine, with which 75·6 grs. of silver can combine, is incapable of entering into combination with more than 3·95598 grs. of the 6·4 grs. of oxygen which that quantity of silver holds in union with it. Or, adopting Mr. J. Davy's analysis of horn silver, the evolution of oxygen should amount to 2·97 cubic inches; because 75·5 grs. of silver hold in combination 6·5 grs. of oxygen, of which only 5·51 grs. can enter into union with the hydrogen (·9224 of a gr.) set free from its combination with the chlorine.

Again, in the process of separating chlorine from silver, there ought to be an evolution of a little more than 15 cubic inches of hydrogen for every 100 grs. because the silver requires 1·44402 gr. of oxygen more than can be furnished by the water sufficient to afford ·677 of a gr. of hydrogen to the chlorine. But, by Mr. J. Davy's analysis, this evolution of hydrogen ought not to exceed 6·99 cubic inches, for 24·5 grs. of chlorine take ·9224 of a gr. of hydrogen, and this quantity is capable of combining with 5·51 grs. of the 6·5 grs. of oxygen required by the silver.

The former part of the reasoning employed to obviate the probable objection against the experiment which I have detailed, would be in some measure incomplete, were I to overlook Sir H. Davy's theory of the nature of the muriates. This I shall notice as briefly as possible.

Taking, then, the instance of muriate of potash, it appears by calculation, that while the oxygen of 100 of potash unites with the hydrogen of 52·4748 of muriatic acid, the residual potassium (86·1) combining with the chlorine (50·16) forms 136·26 of muriate of potash. But the materials employed were in weight 152·4748; so that a deficiency in the weight of the product, of not less than 16·2148, remains to be accounted for. It may, indeed, be said, that on account of the water contained in potash, a deduction is requisite on either theory; but as this operates equally on both, while the other is entirely unnecessary on the old theory, we are thus furnished with an apparently easy mode of discrimination.

Nor does the analysis of the salt seem less decisive: according to Sir H. Davy's theory, 100 parts of muriate of potash should yield 35·976 of muriatic acid, and 71·93 of anhydrous potash; in all, 107·906.

Moreover, in decomposing muriate of potash by nitrate of silver, the potassium must be oxidated by decomposing either the water or the oxide of silver. If by the former means, there must be an evolution of 88·6 cubic inches of hydrogen from the decomposition of 136·26 grs. of the salt. If in the latter way, then, as 50·16 grs. of chlorine can decompose 239·2741 grs. of oxide of silver, the oxygen of this quantity, which amounts to nearly 22·085 grs., must unite with the potassium; but 81·6 grs. of potassium can take 13·9 grs. of oxygen only: therefore there must be an extrication of 8·185 grs. of this gas, or about 24·0735 cubic inches. Beside, the residual muriate of silver will not weigh 291·7489 grs., but 267·3491 grs. only.

Before concluding, I have only to remark, that the calculations above detailed are rendered in a great measure uncertain, from the want of accurate and consistent statements of the composition of potash, and of water, as well as of the quantity of oxygen which may be obtained from a given portion of oxymuriatic gas.

With regard to potash, the various statements given by different chemists, and on different occasions, are full of perplexity.

Sir H. Davy details two experiments, the mean result of which was 13·9 per cent. of oxygen;* and on the basis afforded by this estimate, the calculations in the foregoing paper were made; the method practised in these experiments was the combustion of potassium in oxygen gas, the result of which process is afterwards † said by Sir H. Davy to be peroxide of potassium.‡

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* Phil. Trans. 1808, p. 28.

† Phil. Trans. 1811, p. 6.

‡ Yet the experiments detailed in page 4 of the Phil. Trans. for 1811 seem to indicate that the peroxide of potassium contains about 30 per cent. of oxygen; and MM. Gay-Lussac and Thenard (Recherches, &c. i. p. 132) state that it contains three times as much oxygen as exists in potash.

By the evolution of hydrogen from water by potassium, the proportion is fixed at 16.* At p. 34 of the Phil. Trans. for 1810, is a reference to the Bakerian lecture for 1807, as warranting an estimate of 15·254. On one occasion† calculations are made from a proportion of 14·895, and on another‡ 15·6 is assumed. MM. Gay-Lussac and Thenard give 16·629 as the result of their experiments;§ and M. Berzelius states at first‖ 17·007, but afterwards** either 16·978 or 17·153. It may perhaps be objected to the accuracy of my calculations, that I have founded them on a proportion authorized by the mean result of only two experiments, and those the first that had been performed on the subject. To this I would reply, that the detail given of these experiments appeared to me sufficiently minute and satisfactory; and that the other results have all of them been obtained by calculations founded on a view of the constitution of water, with which I must confess myself not perfectly satisfied.

The statement of the composition of water given by Dr. Thomson,†† on the authority of Fourcroy, Vauquelin, and Sequin, has been adopted in the preceding calculations: this proportion is 85·662 of oxygen to 14·338 of hydrogen. Gay-Lussac and Humboldt give 87·4 and 12·6;‡‡ and in the Recherches Physico-Chimiques, the calculation of the constituents of turpentine §§ is made from the proportion of 87·998152 and 12·001847, while in the analysis of oxalic acid‖‖ the proportions asssumed are 88·367260 and 11·632739. The proportions assigned by Berzelius are 88·246 and 11·754,*** or 87·587 to 12·413.†††

Mr. Murray‡‡‡ gives 16 of oxygen and 84 of muriatic acid as the constituents of oxymuriatic gas: Dr. Thomson §§§ details an experiment by Chenevix, apparently very accurate, the results of which were 22·5 of oxygen, and 77·5 of muriatic acid; and to this estimate I have adhered in the foregoing paper. Mr. Dalton‖‖‖ states 24 of oxygen, and 76 of muriatic acid; and Berzelius**** assigns 23·37 and 76·63, but afterwards†††† states 22·768 and 77·232.

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* Phil. Trans. 1808, p. 30.

† Phil. Trans. 1809, p. 55.

‡ Phil. Trans. 1810, p. 245.

§ Recherches Phys Chim. i. p. 123.

‖ Ann. de Chim. lxxvii. p. 84.

** Ann. de Chim. lxxix. p. 140.

†† System, &c. i. p. 123.

‡‡ Ann. de Chim. liii. p. 248.

§§ Ann. de Chim. ii, p. 313.

‖‖ Ann. de Chim. ii. p. 302.

*** Ann. de Chim. lxxvii. p. 84; lxxxi. p. 24.

††† Ann. de Chim. lxxix. p. 246.

‡‡‡ System of Chemistry, ii. p. 657.

§§§ Syst. of Chem. ii. p. 257.

‖‖‖ New System, &c. 308.

**** Ann. de Chim. lxxvii, p. 84.

†††† Ann. de Chim. lxxx. p. 31.

Article XI.

Memoir on the Determination of the Specific Heat of the different Gases. By MM. F. Delaroche, M.D. and J. E. Berard.*

The subject proposed by the Institute as a prize at the meeting of the 7th January, 1811; namely, the determination ot the specific heat of the gases; had previously attracted the attention of different philosophers, some of whom have treated of it in detail: yet so little progress has been made in the investigation, though the question proposed be sufficiently simple, that we are almost as far from being able to answer it with precision as we were before it became the subject of investigation.

Crawford, as far as we know, is the first person who began the investigation. He published the result of his researches in 1788. At that time a great number of experiments had been made upon the specific heat of bodies in general. MM. Lavoisier and de Laplace had already published the result of their experiments on that subject, and had given a description of their calorimeter of ice: yet Dr. Crawford preferred the method of mixtures of water, or other bodies whose specific heat was considered as known. After many unsuccessful attempts, which it would be too tedious to describe here, he thought that he succeeded by the following method. He procured two copper vessels, very thin, and of the same shape, size, and weight. He filled one of them with the gas that he wished to examine, and made a vacuum in the other. He then heated both in boiling water, and plunged both suddenly into cylinders containing a small quantity of cold water, but sufficient to cover them. He subtracted the heat communicated to this water by the exhausted vessel from that communicated by the vessel full of gas, and considered the remainder as the effect produced by the gas, or as its specific heat. He had taken great precautions to ensure the accuracy of his results; but it is quite evident, from the smallness of the change, that no confidence could be placed in them. The rise in the temperature of the water never exceeded 0·4° Fahrenheit. The following is the table of his results:—

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* Translated from the Annales de Chimie for January, 1813, vol. lxxxv. p. 72. This memoir gained the prize proposed by the Institute, and deserves particular attention. It overturns the theory of animal heat advanced by Crawford, and Lavoisier's theory of combustion.

Specific heat of Water	1·000
Atmospheric air	1·790
Oxygen	4·749
Azote	0·793
Carbonic acid	1·045
Hydrogen	21·400

Before the work of Crawford appeared, Lavoisier and Laplace had made some experiments, which were not published till long after. They had employed their calorimeter of ice, through which they passed a current of gas contained in a serpentine, which enveloped on all sides the ice of the interior chamber. A thermometer placed at each extremity of the serpentine enabled them to observe the temperature of the gas when it entered and came out of the calorimeter. The gas was heated by passing through a serpentine surrounded with boiling water, before it entered into the calorimeter. These experiments, though susceptible of much greater precision than those of Crawford, were not free from very material imperfections. The method employed by these philosophers to take the temperature of the gases at its entrance into the calorimeter was insufficient, since the gas, in passing through the exterior coating of ice, would lose a portion of the heat which the thermometer had indicated, without contributing by that to melt the ice in the interior chamber. On the other hand, they do not say that they took any precautions to dry the gases upon which they made their experiments. These gases, charged with the humidity which the contact of water in the gazometers would necessarily communicate, no doubt deposited the whole of it when they passed through the calorimeter; but we know that vapour, when it condenses, gives out a great deal of heat. It is proper, however, to remark, that the temperature at which these experiments were made, being probably but little elevated above the freezing point, the quantity of vapour mixed with the gas would not be considerable. Lavoisier and de Laplace only subjected to these experiments oxygen and atmospherical air. They found for the specific heat of the first (that of water being 1·00), 0·65, and for the second 0·33; but Lavoisier acknowledges that the accuracy of these results cannot be entirely depended on.

Since that time, various attempts have been made to appreciate, by indirect means, the specific heat of some gases. Mr. Leslie has employed, in order to compare the specific heat of hydrogen and atmospheric air, a process founded on the following considerations. When a large receiver is partly exhausted of air, if air be allowed to enter into it, the dilated air which it contained will condense, and its temperature will be increased by a constant quantity, whatever gas enter into it; but the entering gas will absorb a part of this excess of heat, and the mixture will have a mean temperature between that of the entering gas and that which the air would have acquired if it had not been obliged to part with any of its heat. Now it is evident that this mean temperature will be so much the lower the greater the specific heat is of the gas which enters. The experiments made by Mr. Leslie have led him to conclude, that equal volumes of hydrogen and atmospherical air have the same specific heat.

The principle upon which this ingenious process is founded is not perfectly just; since it appears from the experiments of Gay-Lussac,* that part of the heat developed in this case comes from the gas that enters into the receiver. It appears, likewise, that some unknown circumstance has misled Mr. Leslie respecting the result of his experiments: for analogous experiments, made with the greatest care, have given different results to Gay-Lussac; and he did not observe that equality of effect, which takes place, according to Mr. Leslie, when atmospherical air and hydrogen gas are made to enter into an exhausted receiver. Gay-Lussac, in these experiments, made use of two similar balloons, communicating with each other by a pipe furnished with a stop-cock. He made a vacuum in the one, and filled the other successively with different dried gases. In the centre of each of these balloons was a thermometer of spirit of wine. When the stop-cock was turned, the gas rushed from the full vessel into the empty one. He had taken precautions to render the velocity of the current equal in all cases. The thermometer of the first balloon sunk, and that of the second rose the same number of degrees; but that number varied according to the nature and density of the gases employed. Gay-Lussac, thinking that the specific heat of the gases subjected to these experiments was proportional to the rising and falling of the thermometers, thought himself entitled to conclude that the specific heat of equal volumes of the different gases was inversely as their specific gravity, and of the same gas directly as its density; but he only gave this opinion as a probable conjecture, without affirming any thing respecting its justness. In fact, the phenomena which take place in such cases are very complicated, and it is almost impossible to distinguish what depends upon the different conducting power of the gases from what depends upon their specific heat.

M. Gay Lussac has discovered this himself, in the new memoir which he has lately published on the subject,† and in which he came to different results. He employed in these last experiments a very simple and ingenious method to determine the specific heat of the gases. It consists in passing to the centre of a small reservoir, containing a thermometer, a current of two different gases, the one hot, the other cold. Knowing the temperature of the two gases before their mixture, and that of the mixture, it was easy to infer the ratio between their respective specific heats. This process, besides that it does not give us the ratio between the specific heat of the gases and that of water, is attended with another inconvenience. When only small quantities of gas are employed, a great part of their heat is communicated to the vessels in which the mixture is made, which may lead to erroneous conclusions. Accordingly, as he made his experiments at first on the gases that can be most conveniently procured, such as air, hydrogen, carbonic acid, &c. which, as will be seen afterwards, do not differ much in their capacity for heat, he was induced to believe that the same volumes of all the gases had the same capacity. However, he afterwards published a note,* from which we see that he had brought his process to perfection by operating upon large quantities of gas. By that method he ascertained that hydrogen and carbonic acid have different specific heats, and the numbers which he assigns approach to those which we have ourselves obtained. This induces us to remark to the commissioners, that a first memoir having for its motto *Tectus magis æstuat ignis*, which contains our most important results, was deposited in the hands of the Secretary of the Institute on the 3d of February, 1812, more than five months before that note of Gay-Lussac was published in the Annales de Chimie.

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* Mem. d'Arcueil, i. 180.

† Ann. de Chim. lxxxi. 98.

Among the attempts made to determine the specific heat of the gases, we ought to reckon the table drawn up by Mr. Dalton from principles purely theoretical, founded on this hypothesis, that the quantities of heat belonging to the ultimate particles of all elastic fluids ought to be the same under the same pressure and at the same temperature. His table is as follows :—

Hydrogen gas	9·382	Nitrous gas	0·777
Azotic gas	1·866	Oxide of carbon	0·777
Atmospheric air	1·759	Vapour of alcohol	0·586
Ammonia	1·555	Sulphureted hydrogen	0·583
Olefiant gas	1·555	Nitrous oxide gas	0·549
Oxygen	1·333	Vapour of nitric acid	0·491
Carbureted hydrogen	1·333	Carbonic acid	0·491
Aqueous vapour	1·166	Muriatic acid	0·424
Vapour of ether	0·848		

Such are the results of the investigation of this subject hitherto made. By comparing them together, it is easy to see how fur-they differ from one another. Unless we deceive ourselves respecting the justice of those results which we ourselves have obtained, it will be seen, by what follows, how far they all deviate from the truth.

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* Ann. de Chim. lxxxiii. 106.

(To be continued.)

Article XII.

Astronomical and Magnetical Observations at Hackney Wick.

By Col. Beaufoy.

If the wires of a transit instrument be placed at unequal distances from the centre wire, and from each other, the following method may be used to correct the mean of the times of the sun or star passing the lateral wires, (*i. e.*) those on each side of the middle one.

Let a, b, c, d, represent the equatorial intervals, or the time that a star in the equator takes to pass from wire to wire; and suppose x to be the time that the star passes the first or easternmost wire; then x + a is the time of passing the second wire; x + a + b, the time of passing the third wire; x + a + b + c, the time of passing the fourth wire; and x + a + b + c + d, the time of passing the fifth or last wire.

If the wires had been equi-distant, it is evident half the sum of the first and fifth, or second and fourth, would have been equal to x + a + b, or $\frac{1}{5}$ of the times of passing all the wires; therefore the differences between each, and x, a, b, will be the required corrections.

Suppose the equatorial intervals of a, b, c, and d, to be as follows: a = 25·33″, b = 25·07″, c = 25·87″, d = 25·67″, then x + 50·40″ is equal to x + a + b, the time of passing the middle wire; but a mean of all the five wires is a + 50·78″: therefore the correction is 0·38″ to be subtracted.

Again, the sum of the first and fifth wires is 2x + 101·93″, its half is x + 50·97″, and the correction 0·57″ to be subtracted. Half of the sum of the second and fourth wires is x + 50·80″, the correction 0·40″ to to be subtracted.

a = 25·33″
b = 25·07
c = 25·87
d = 25·67

x	Time of passing the 1st wire.
x + 25·33″	Ditto............2d
x + 50·40	Ditto............3d
x + 76·27	Ditto............4th
x + 101·93	Ditto............5th

$\frac{2x + 101{\cdot}93''}{2} = x + 50{\cdot}97''$ Mean of the 1st and 5th wires.

$\frac{2x + 101{\cdot}60''}{2} = x + 50{\cdot}80$ Ditto..2d and 4th.

$\frac{4x + 203{\cdot}53''}{4} = x + 50{\cdot}88$ Ditto..1st, 2d, ~~3d,~~ 4 and ~~4th.~~ 5

$\frac{3x + 152{\cdot}00''}{3} = x + 50{\cdot}67$ Ditto..2d, 3d, and 4th.

$\frac{3x + 152{\cdot}33''}{3} = x + 50{\cdot}78$ Ditto..1st, 3d, and 5th.

$\frac{5x + 253{\cdot}94''}{5} = x + 50{\cdot}79$ Ditto..1st, 2d, 3d, 4th, and 5th.

By taking the differences between the time that the star passes the middle wire, and the mean of the times of passing the other wires, the following six corrections are obtained:—

Subtract 0·57″ for the 1st and 5th wires.
Subtract 0·40 for the 2d and 4th wires.
Subtract 0·48 for the 1st, 2d, ~~3d, and~~ 4th wires. & 5th
Subtract 0·27 for the 2d, 3d, and 4th wires.
Subtract 0·38 for the 1st, 3d, and 5th wires.
Subtract 0·39 for the 1st, 2d, 3d, 4th, and 5th wires.

Should the star or sun not be in the equator, these corrections must be multiplied by the natural secant of the object's declination; or to the logarithm of the equatorial correction add the log. secant of the declination; the natural number answering to the sum will be the required correction.

The angles subtended by the spaces between the wires may be found by placing different marks at a given distance from the instrument, and from each other, and then calculating the angles; or they may be found by observing, with a watch or clock, the seconds and parts of a second the stars, situated in Arion's Belt and Sword, take to pass from wire to wire.

N.B. Should a star be observed under the Pole, the correction must be added, not subtracted.

Astronomical Observations.

Hackney Wick.

July 12, Immersion of π Sagittarius at ..12^h 56′ 16″ Mean Time. Certain to a second, and no diminution of the star's light took place at the contact.

Magnetical Observations.

Latitude 51° 32′ 40″ North. Longitude West in Time 6 $\frac{82}{100}$.

1813.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
June 19	8h 40′	24° 14′ 12″	1h 40′	24° 23′ 40″	7h 35′	24° 14′ 34″
Ditto 20	8 15	24 11 05	1 25	24 23 24	6 52	24 17 52
Ditto 21	8 44	24 13 16	1 25	24 21 14	7 30	24 15 45
Ditto 22	8 35	24 14 55	1 15	24 22 42	7 50	24 16 54
Ditto 23	8 35	24 12 57	1 52	24 20 53	7 25	24 14 25
Ditto 25	8 37	24 13 52	2 00	24 20 54	8 00	24 15 21
Ditto 27	8 25	24 14 38	1 30	24 23 42	— —	— — —
Ditto 28	8 35	24 14 01	1 30	24 23 57	7 13	24 16 21
Ditto 29	8 22	24 10 37	1 20	24 24 32	6 32	24 14 48

Mean of Observations in June.	Morning	at	8h 30′.....	Variation	24°	12′	55″
	Noon	at	1 33	Ditto	24	22	17
	Evening	at	7 04	Ditto	24	16	04
Ditto in May..	Morning	at	8 28	Ditto	24	12	02
	Noon	at	1 37	Ditto	24	20	54
	Evening	at	6 14	Ditto	24	13	47
Ditto in April	Morning	at	8 31	Ditto	24	09	18
	Noon	at	0 59	Ditto	24	21	12
	Evening	at	5 46	Ditto	24	15	25

Magnetical Observations continued.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
July 1	8h 10′	24° 11′ 15″	1h 35′	24° 22′ 30″	7h 20′	24° 15′ 50″
Ditto 2	8 35	24 13 53	1 39	24 21 17	— —	— — —
Ditto 4	8 37	24 14 44	1 40	24 23 47	8 20	24 15 47
Ditto 5	8 30	24 11 58	1 45	24 21 01	7 05	24 16 09
Ditto 6	8 35	24 15 56	1 38	24 24 00	7 25	24 15 35
Ditto 7	8 27	24 13 37	— —	— — —	7 17	24 15 24
Ditto 8	8 40	24 14 11	1 52	24 24 10	6 55	24 16 14
Ditto 9	8 45	24 12 47	— —	— — —	7 40	24 16 53
Ditto 10	8 10	24 15 25	1 50	24 21 06	7 55	24 15 05
Ditto 11	8 33	24 14 35	1 35	24 21 55	7 10	24 15 50
Ditto 12	8 30	24 14 55	— —	— — —	— —	— — —
Ditto 14	— —	— — —	— —	— — —	6 58	24 17 56
Ditto 15	8 30	24 15 20	— —	— — —	— —	— — —
Ditto 16	8 40	24 15 02	1 50	24 23 38	— —	— — —
Ditto 17	8 30	24 13 34	1 55	24 23 38	7 15	24 18 22
Ditto 18	8 30	24 15 10	1 53	24 22 25	7 42	24 16 52

ARTICLE XIII.

ANALYSES OF BOOKS.

Tableau Methodique des Espèces Minerales, Seconde Partie; contenant: la Distribution methodique des Espèces Minerales, extraite du Tableau Cristallographique publié par M. Haüy en 1809, *leur Synonymies Française, Allemande, Italienne, Espagnole, et Anglaise, avec l'Indication de leur Gisment; auxquelles on a joint la Description abregée de la Collection de Minereaux du Museum d'Histoire Naturelle et celle des Espèces et des Varietés observées depuis* 1806, *jusq'en* 1812, *par I. A. H. Lucas, adjoint a son Père, Garde des Galeries du Museum d'Histoire Naturelle et Agent de l'Institut Imperial de France, Membre de plusieurs Societés Savantes. Paris,* 1813.

This book is chiefly interesting because it shows us the present state of mineralogy in the French capital, and makes us acquainted with the additional facts ascertained by Haüy (it would appear the only mineralogist in Paris) since the publication of his *Tableau Comparatif* in 1806. As the French mineralogists confine themselves chiefly to crystallized minerals, and are not acquainted with the method of describing minerals by their external characters, it is hardly possible to make their labours intelligible, except by figures. And as Lucas gives no figures, the utility of his work is in a great measure confined to those persons who are employed in studying mineralogy under Haüy. The localities of the different minerals which he has added are likewise of considerable value; and the notes copied from various new publications, some of which are unknown in Great Britain, give the work an additional value to the English reader. Instead of a regular analysis of this book, which would be of no use, I shall endeavour to collect from it the improvements which Haüy has introduced into his system since the original publication of his *Mineralogie* in 1801.

I. Haüy, as is known, I presume, to most of my readers, has invented a new name for almost every mineral species. These innovations originating, it is difficult to conceive, from what cause, have been highly approved of by the French in general. And M. Lucas, to enable foreigners to participate in the blessing of these new names, has been at the trouble to get the Haüyan nomenclature translated into German, Italian, Spanish, English, and Latin. The English translator, he tells us, is Dr. Russel, we presume an American. Unluckily, the translation is so little accommodated to our language, that it could not be used without exciting ludicrous ideas. The adjective being always placed after the substantive, a position which our language does not admit. Thus we have *lime carbonated*, *lime phosphated*, *lime fluated*, *lime sulphated*, &c. If the other translations are not more fortunate than this, neither the Germans, Spaniards, nor Italians, will derive much advantage from the benevolent exertions of M. Lucas.

The nomenclatural zeal of M. Haüy is far from being cooled. In his mineralogy he allowed about ten of the old names of stony minerals to remain; among others, *felspar*, which had been employed from time immemorial, was permitted to figure away among the new words. This word has been since discarded, and the word *orthose* invented and substituted in its place.

II. The following are the new species admitted by Haüy into his system since the publication of his *Mineralogie*:—

1. Sulphuric acid. This acid was first observed in a lake in Italy in 1776. It has been since found in South America by Humboldt, and in a lake in Java by Leschenault de la Tour.

2. Boracic acid. Found in different lakes in the country of Sienna, in Italy, in 1776. It exists also in Thibet; at least this opinion is entertained. The lakes of that country yield the borax of commerce.

3. Carbonic, sulphurous, and muriatic acids.

4. Ferruginous carbonate of lime. Found in Spain, and at Saltzburg. Romé de Lisle confounded the crystals of this variety of calcareous spar with those of fluor spar.

5. Red manganesian carbonate of lime. It has a red colour, and the crystalline form of carbonate of lime. It is a mixture of carbonate of lime, carbonate of manganese, and quartz.

6. Pearly carbonate of lime. This is the schieferspar of Werner, and the argentine of Kirwan.

7. Arragonite. Haüy threw this species of carbonate of lime into his appendix, on account of its crystalline form. He has since introduced it into his system as a distinct species.

8. Silicious phosphate of lime. This variety was described by Brogniart in his Mineralogy, and seems to be a mixture of phosphate of lime and quartz.

9. Ferruginous sulphate of magnesia. This is the haarsaltz of Werner. It occurs in abundance in the old coal-pits near Glasgow, from which alum is made.

10. Silicious carbonate of magnesia. This is the *native* magnesia of Werner discovered by Dr. Mitchell.

11. Silicious borate of lime. This is the *datholite* discovered at Arendal, in Norway, by Esmark, in 1806.

12. Sulphate of soda. Found in many mineral waters. It occurs, likewise, in Germany, crystallized, and in powder.

13. Sulphate of ammonia. Found on Mount Vesuvius and Mount Etna.

14. Glauberite. Found in Spain. It is a mixture or combination of sulphate of lime and sulphate of soda.

15. Corundum. Under this name Haüy at present comprehends his old species telesia, corundum, and emery; which Count Bournon demonstrated to belong to one species.

16. Tenaceous felspar Under this name is described the mineral called jade by Saussure, and saussurite by Karsten.

17. Apophyllite. This is the ichthyophthalmite of d'Andrada.

18. Triphane. This is the spodumene of d'Andrada. Both these species were found in Sweden, and have been for these eight years well known to mineralogists.

19. Yenite. This is a very rare mineral hitherto found only in the Isle of Elba, and discovered by Lelievre in 1801. As it contains more than half its weight of iron, one should have expected that Haüy would have classed it rather with the iron ores than with the stony minerals.

20. Hyperstene. This is the mineral formerly known by the name of Labradore hornblende, which Haüy has lately constituted a species apart.

21. Paranthine. This is the scapolite of d'Andrada, well known to mineralogists for these eight years.

22. Lomonite. This mineral, discovered by Gillet Laumont, has been for several years constituted a peculiar species by Werner.

23. Cubo-octahedral analcime. This is the mineral called *sarcolite* by Dr. Thompson, of Naples. Haüy considers it as a variety of his analcime.

24. Pinite. This is the micarelle of Kirwan. It has been long known to mineralogists.

25. The appendix to the stony minerals contains the following substances, respecting the nature of which Haüy has not yet come to any determination. Allochroite, pure alumina, amianthoide, anthophyllite, aplome, bergmannite, diaspore, felspar *apyre*, blue felspar, fibrolite, gabbronite, jade, iolite, cinnamon stone, lazulite of Werner, latialite, lepidolite, melilite, natrolite, fatstone, pseudosommite, tabular spar, spinellare, automolite, spinthere, talc.

26. The additional species in the class of ores are not very numerous. They are as follows:—

Carbonate of silver.
Red oxide of lead.
Black carbonate of lead.
Cupriferous carbonate of lead.
Arseniferous phosphate of lead.
Native nickel.

Argentiferous arsenical nickel.
Arseniferous grey copper ore.
Antimoniferous grey copper ore.
Platiniferous grey copper ore.
Dioptase or copper emerald.
Native iron.
Titaniferous oxide of iron.

White sulphuret of iron. This is the pyrites known in this country by the name of cockscomb pyrites. It has often a green colour externally, but is always of a very light yellow when broken. It constitutes a distinct species, and probably is composed of different proportions of the constituents from common pyrites.

Phosphate of iron.
Arseniate of iron.
Carbonate of zinc.
Sulphuret of manganese.
Ferriferous phosphate of manganese.
Anatase.
Oxide of collumbium.
Allanite, or siliciferous oxide of cerium.
Oxide of chromium.

III. A great number of additional crystalline forms has been determined by Haüy. This is the branch of mineralogy in which Haüy's great excellence consists. All his remarks on it are valuable, and entitled to great attention. As he is himself printing at present a second edition of his work, which will make its appearance in a very short time, and as Lucas gives us no figures, and indeed describes no crystals except those noticed by Haüy in his *Tableau Comparatif*, it is unnecessary to notice these additions here. The number of forms of calcareous spar made out by Haüy at present amount to 150. The forms of sulphate of barytes are not much fewer.

IV. M. Lucas terminates his book with a table of the distribution of rocks, according to the system of M. Tondi. It is obviously founded upon the geognosy of Werner: from which, indeed, it differs chiefly in the adoption of a great variety of new words to accommodate it to the present French fashion; and in the introduction of a few rocks not noticed by Werner. I shall endeavour to give the reader an idea of this arrangement. For the term *geognosy* Tondi substitutes the word *oreognosy*, a faulty appellation, because the object is not to make us acquainted with the structure of mountains, but with that of the whole crust of the earth. Oreognosy he divides into three branches; namely, *oreogenese*, or the formation of rocks; *oreotectonique*, or the structure of rocks; and *oreodiacritique*, or the distribution of rocks. With respect to the first two of these branches, we have no details; but a table is given of the distribution of rocks. They are divided into six classes, namely:—

I. *Mountains in Mass.*

They are all composed of the oldest granite.

II. *Mountains in Beds.*

1. Second formation of granite.
2. Whitestone. Felspar with garnet, or mica, or cyanite, or hornblende.
3. Gneiss.
4. Mica slate.
5. Clay slate.
6. Topaz rock.
7. Primitive calcareous spar.
8. Dolomite.
9. Flinty slate.
10. Serpentine.
11. Greenstone.
12. Magnetic ironstone.
13. Iron glance.
14. Cyanite.
15. Porphyry.
16. Amygdaloid.

III. *Transition Rocks.*

1. Grey wacke.
2. Transition limestone.
3. Transition greenstone.
4. Transition amygdaloid.
5. Transition flinty slate.
6. Ribbon jasper.
7. Red iron ore.
8. Transition sienite.
9. Transition felspar porphyry.
10. Transition granite.
11. Transition sandstone.

IV. *Floetz Rocks.*

1. Conglomerate.
2. Old red sandstone.
3. Bituminous marl slate.
4. Indurated marl.
5. Compact limestone.
6. Second compact limestone.
7. Amygdaloidal limestone.
8. Foliated gypsum.
9. Common salt.
10. Second sandstone.
11. Fibrous gypsum.
12. Third limestone.
13. Calamine.
14. Fourth limestone.
15. Third gypsum, or plaster of Paris.
16. Third sandstone.
17. Chalk.
18. Geanthrace, or glancecoal.
19. Phytanthrace, or black coal.
20. Zoo-phytanthrace, or black coal.
21. Friable sandstone.
22. Alum slate.
23. Slate clay.
24. Bituminous shale.
25. Cinnabar.
26. Jasper form clay ironstone.
27. Clay ironstone.
28. Common clay.
29. Claystone.
30. Clay porphyry.
31. Hornstone.
32. Lithomarge.
33. Marl.
34. Compact limestone.

35. Tripoli.
36. Basalt.
37. Greenstone.
38. Clay porphyry.
39. Wacke.
40. Green earth.
41. Yellow earth.
42. Greystone.
43. Phonolite, or clinkstone.
44. Amygdaloid.
45. Trap tuff.
46. Pitchstone.
47. Obsidian.
48. Semiopal.
49. Sand.
50. Sandstone.
51. Slate clay.
52. Common clay.
53. Compact limestone.
54. Clay marl.
55. Common clay ironstone.
56. Chromate of iron.
57. Brown coal.

V. *Mountains of Alluvion.*

1. Sand.
2. Clay.
3. Calctuff.
4. Moorland.

VI. *Volcanic Mountains.*

* Pseudo-volcanos.

1. Scorias.
2. Porcelain jasper.
3. Polishing slate.
4. Clay ironstone.
5. Native steel.
6. Bitumen, &c.
7. Hot water.
8. Sulphur.
9. Silicious tuff.
10. Muriate of ammonia.

** Volcanos.

I. *Substances thrown out unaltered.*

1. Augite.
2. Garnet.
3. Vesuvian.
4. Meionite.
5. Leucite.
6. Sommite.
7. Dolomite.
8. Mica.
9. Meteoric iron.
10. Water.

II. *Substances Melted.*

1. Lava.
2. Volcanic tuff.
3. Moja.

III. *Substances Sublimed.*

1. Common salt.
2. Sulphate of ammonia.
3. Sal ammoniac.
4. Iron glance.
5. Red muriate of iron.
6. Muriate of copper.
7. White oxide of arsenic.
8. Realgar.
9. Sulphur.

Notwithstanding the great length of this list, I have omitted all the subordinate formations, which are very numerous. The great mistake into which Tondi appears to have fallen in the preceding classification is the confounding together, as of equal importance, those substances which occur in great abundance, constituting rocks, or mountains of considerable extent, with those substances which occur only in minute quantities scattered through the first. No classification can be of much value to the practical geognost, unless this distinction is carefully attended to.

Article XIV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Galvanic Battery.*

On Saturday, the 2d of July, J. G. Children, Esq. put in action the greatest galvanic battery that has ever been constructed. It consisted of 20 pair of copper and zinc plates, each plate 6 feet in length, 2 feet 8 inches in breadth Each pair was fixed together at the top by pieces of lead cut into ribbons. A separate wooden cell was constructed for each pair. The plates were suspended from a wooden beam fixed at the ceiling, and were so hung by means of counterpoises that they could be easily raised or let down into the cells. The cells were filled with water, containing a mixture of sulphuric and nitric acids. At first the acids amounted to 1-60th of the water; but more was gradually added till it amounted to the 30th. Leaden pipes were attached to the two extremities of the battery, and conveyed the electricity out of doors to an adjoining shade, where the experiments were made. The power of this battery was very great; though I am not certain whether it increased in proportion to the size of the plates. It ignited about 6 feet in length of thick platinum wire. The heat produced was very intense. It melted platinum with great facility. Iridium was likewise melted into a globule, and proved to be a brittle metal. The ore of iridium and osmium was likewise melted, but not so completely. Charcoal was kept in a white heat in chlorine gas, and in phosgene gas; but no change took place in either of these gases. Neither tungsten nor uranium underwent any change. A very singular fact was pointed out by the sagacity of Dr. Wollaston, and succeeded upon trial. A greater length of thick platinum wire was ignited than of platinum wire of a much smaller size. This Dr. Wollaston had previously ascertained in his own minute galvanic batteries, consisting of a single pair of small plates.

II. *Volatility of Cerium.*

The volatility of this metal, which had been previously inferred from the experiments of Vauquelin, was fully confirmed in Mr. Children's laboratory at Tonbridge. A quantity of oxalate of cerium had been prepared in order to obtain from it the oxide of the metal. This oxalate was exposed in a charcoal crucible within a tobacco-pipe mouth to the strongest heat that could be raised in a forge. In this heat the cerium was volatilized so completely that not the least trace of it remained.

III. *Action of the Agate on Light.*

Dr. Brewster has now established, by numerous experiments, that the nebulous light of the agate has the same relation to the bright image as the extraordinarily refracted image has to the ordinarily refracted image of all doubly refracting crystals. There is still, however, no appearance of the nebulous light being produced by a higher refractive power than the bright image. All the phenomena of polarization are produced when the plate of agate is less than the $\frac{1}{150}$th of an inch; and the nebulous light, when in its evanescent state, can be revived by depolarization with mica, in every respect like one of the images formed by double refraction. The coloured appearances in the agate, to which mineralogists have given the name of iridescence, furnish a series of the most singular results, arising, apparently, from the mechanical structure of different laminæ, and seem to open up a new field in physical optics. He is at present examining various specimens, in order to give a greater degree of generality to the results previous to laying them before the public.

IV. *Transition Rocks in Yorkshire.*

I have received the following letter, which I lay before the public; because I consider every fact that adds to our knowledge of the mineralogical structure of Great Britain as interesting. I have myself seen most of the places referred to in the letter, but had no opportunity of determining the structure of them with any thing like accuracy; nor was I aware that any transition formations occurred upon the east coast of Yorkshire.

SIR,

In the *Annals of Philosophy*, No. VI. Article II. "On Transition Rocks," the writer mentions, that he "knows of no transition rocks to the *east* of a line extending almost due south from Berwick to the English Channel."

Knowing from ****, and others, that you have visited Yorkshire, and given particular attention to the geology of the county, I presume that you are well acquainted with the under stratum of the Wolds, and therefore that the lines I have quoted have escaped your observation, or would have been accompanied with your remark, noticing the transition limestone of that district.

The chalky limestone stratum of the Wolds, of which Flambro' Head is the most easterly point, and Leavening Brow the most westerly, is of very considerable depth, as the cliff from Flambro' to Spector, and the wells that have been made for obtaining water, indubitably prove. The cliff at Spector appears to the eye at least 400 feet perpendicular, and is one entire bed of the same stone, not intersected by any veins whatever, and containing various shells wherever a quarry is opened in this extensive stratum.

Flambro' Head and Filey Bridge are the two most easterly points of the bay generally called Filey Bay. From Flambro' Head to Spector the white cliffs of Albion appear for the last time in sailing to the North; and at Filey Bridge the dark colour of the moorstone district begins, which continues to the most northerly point of Britain.

Filey Bridge has been formed by the Ocean gradually washing off an immense bed (100 feet thick) of argillaceous earth from the rocky base, which dips in a southerly direction; and in the highest part is not more than 50 or 60 feet above high water mark. This base I call a firm argillaceous sandstone; it is in large blocks, containing shells, and intersected by grey limestone in thin strata, from 6 to 18 inches, containing also various shells, particularly the large ostrea, and dipping at the same angle, and in the same direction; the angle about 15 degrees.

Should these observations recal to your memory the minutes you would of course make in your geological view of the East Riding of Yorkshire, perhaps you will favour the county with a publication of them, at some convenient opportunity, in the *Annals of Philosophy*. This, I am sure, will oblige your Yorkshire friends; and particularly, Sir,

Your most obedient servant,

July 7, 1813. I. W. M.

To Dr. Thomson, M.D. &c. &c.

V. *Structure of the Cube.*

The following letter deserves to be laid before the public, because it forms what I consider as a valuable addition to Dr. Wollaston's ingenious explanation of the structure of crystals from spherical particles of matter.

SIR,

Dr. Wollaston, in the Bakerian Lecture published in the Philosophical Transactions, shows very ingeniously in what manner the regular tetrahedron and octahedron may be formed of spherical particles.

In addition to the figures there stated, I should wish to present those Gentlemen, who have turned their attention to the simple and beautiful theory of spherical particles, with a cube to be extracted from a tetrahedron, similar to that described in the Lecture, capable of being held together by the most simple power of attraction, and possessing four pair of poles, which possibly may account for the phenomena discoverable in the boracite.

If from a tetrahedron consisting of seven layers a tetahedron be taken from each solid angle of three layers deep, then will remain an octahedron having four spheres in each edge, and a single one in the centre of each face, which is the corner of a cube contained in the octahedron; and should five spheres be removed from each solid angle of the octahedron,* a cube will remain, constructed exactly upon the same principle with the tetrahedron and octahedron.

The easiest way to make this cube is to take a tetrahedron consisting of ten spheres, and place one sphere on the centre of each face, which will produce exactly the same figure.

Should you think the above account of sufficient importance, your insertion of it in the *Annals of Philosophy* will great oblige,

Sir, your most obedient servant,

N. I. Larkin.

Article XV.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

On Thursday the 1st of July a paper by Sir Humphry Davy was read, containing farther observations on the new detonating compound of chlorine and azote. After recovering from the accident which happened to him during his original experiments on this substance, Sir Humphry made a variety of experiments to determine its properties and composition. Its specific gravity is 1·623. When in contact with water, it congeals at about 40°; but this does not happen when it is kept separate from water. It detonates in nitric acid, and in ammonia; gives out azote in muriatic acid, and is likewise decomposed in sulphuric acid. Attempts were made to decompose it in exhausted glass vessels in the state of vapour; but they were unsuccessful. In general, the glass was broken by an explosion; and when that did not happen, the proportion of chlorine and azote evolved could not be determined, on account of the unknown proportion of atmospheric air that remained in the vessel. When the detonating compound is brought in contact with mercury, a white powder is formed, and azotic gas disengaged. In one experiment a detonation took place, which obliged him to work upon smaller quantities. The white powder was found to be a mixture of calomel and corrosive sublimate; and it sublimed entirely without the disengagement of any gas, indicating the absence both of hydrogen and oxygen. Muriatic acid does not destroy the colour of solution of indigo in sulphuric acid; but if it be impregnated with chlorine, it destroys a determinate quantity of the blue colour, according to the proportion of chlorine present. The same thing happens when the detonating compound is dissolved in muriatic acid. This furnished a method for determining the proportion of chlorine contained in the detonating compound. The result of all these methods of analysis is, that the detonating compound is composed of

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* The removal of the solid angles of the octahedron is only for the purpose of demonstration, and is by no means intended to convey an idea that any natural crystal of this shape is inclined to split in that direction, the impossibility of which has been ably proved by Dr. Wollaston.

Chlorine	91
Azote	9
	100

reckoning by weight, or if we reckon by bulks of

Chlorine	400
Azotic gas	100
	500

Sir H. Davy proposes to call this detonating compound *azotane*.

At the same meeting were read some observations on a new comet, observed by Capt. Hill, in the Hon. East India Company's service.

On Thursday the 8th of July the following papers were read:

1. A catalogue of the positions of a number of circumpolar stars by the Astronomer Royal, Mr. Pond.

2. An analysis of a substance thrown out of Mount Vesuvius, by James Smithson, Esq. This substance had been sent to Mr. Smithson when in Italy in 1794, in order to determine its nature; and he ascertained, by a number of trials, that it consisted chiefly of sulphate of potash. This result was published soon after in an Italian Journal, but no subsequent notice was taken of it by mineralogists. Mr. Smithson was induced to examine it with more accuracy lately, and the result of his experiments is, that it consists of sulphate of potash, sulphate of soda, sulphate of ammonia, muriate of ammonia, muriate of copper, and muriate

of iron, with some earthy matter. Mr. Smithson, by way of introduction to his paper, gives a view of his opinions about the origin of the earth. In his opinion, it was originally a sun, or a comet, and was brought to the state in which it is at present by undergoing combustion at the surface. The volcanoes are the relics of this original combustion, and the materials were the metallic bases of the earthy substances of which the primitive strata are composed. As a proof that these primitive strata have been formed by combustion, he mentions that garnets, hornblende, and other crystals found in them, contain no water, and that little or no water is to be found in the primitive strata themselves.

3. Observations by Dr. Marcet on the cold produced by the evaporation of sulphuret of carbon. This liquid evaporates more rapidly than any other, and produces in consequence a greater degree of cold. A spirit of wine thermometer, having its bulb surrounded with cotton cloth or lint, if dipped into sulphuret of carbon, and suspended in the air, sinks from 60° to 0. If it be put into the receiver of an air-pump, and a moderate exhaustion be made, it sinks from 60° to — 81° (I have seen it myself in these circumstances sink from 74° to — 72°). If a tube containing mercury be treated in the same way, the mercury may be readily frozen, even in summer. The drier the air in the receiver is, the more easily is the cold produced. Hence the presence of sulphuric acid may be of some little service in removing the vapour from the air in the receiver previous to exhaustion; otherwise it occasions no increase of the cold.

4. Observations on the composition of fluor spar, and on its acid basis, by Sir Humphry Davy. The author begins his paper with an historical detail of the attempts made by himself, and Gay-Lussac and Thenard, to decompose fluoric acid, and the ill success of these attempts. It appears from the compounds into which fluoric acid enters, that the weight of an integrant particle of it does not exceed 1·05, supposing an atom of oxygen to weigh 1. Hence it follows that if it is a compound of oxygen and an inflammable base, the base can only have the 20th part of the weight of the oxygen. This supposition he considers as unlikely to be correct. He therefore supposes that fluoric acid, like muriatic acid, is a compound of hydrogen and an unknown supporter of combustion, to which he gives the name of fluorine. He relates many experiments made in order to obtain fluorine in a separate state, but none of them were attended with success. As chlorine has the property of decomposing several oxides, and driving off their oxygen, it occurred to him as likely that it might in certain cases drive off fluorine. Fluate of silver and fluate of mercury, with this view, were acted upon by chlorine. The fluoric acid (or fluorine) was disengaged, and horn silver and corrosive sublimate formed; but no fluorine was

set free. When the experiments were made in glass retorts, the glass was corroded, and silicated fluoric acid gas obtained. When they were made in platinum vessels, the metal was corroded, and a red or brown powder formed. It would seem from the trials made that fluorine has so violent an action on all other bodies that it is very difficult, if not impossible, to obtain it in a separate state. The author promises to continue this subject in a subsequent paper.

5. A paper by Mr. Alman on the method of freeing equations from surds, though the roots be pretty high in their dimensions. From the nature of this paper it could not be read at full length; so that it is not possible to give any idea of the method employed by the author; but he referred to a method previously given in the Irish Transactions by Mr. Money, which he considers as general, and which probably constitutes the foundation of his own.

The Society adjourned till the 4th of November next.

GEOLOGICAL SOCIETY.

On the 2d of April part of a paper by Mr. John Farey, sen. of the Ashover denudation in the county of Derby was read. This part of the paper is not susceptible of abridgment.

On the 23d of April a notice by the Rev. J. J. Conybeare, relative to the slate of Tintagel, in Cornwall, was read. This slate would appear to be transition slate. It answers well for roofing slate, and is traversed by veins containing quartz, calcareous spar, chlorite, and adularia.

Mr. Farey's paper on the Ashover denudation was concluded. The structure of Derbyshire, according to Mr. Farey, consists of alternate formations of limestone and toadstone, in the following order, beginning with the lowest:—

Limestone,
Toadstone,
Limestone,
Toadstone,
Limestone,
Toadstone,
Limestone,
Limestone shale,
Millstone grit,
Coal.

On the 7th of May part of a paper, by Dr. Macculloch, on the geology of certain parts of Scotland, was read. The principal and fundamental rock of the island of Jura is a rock called by some granular quartz, by others granite, and by all considered as a primitive rock; but Dr. Macculloch considers it as a mechanical *deposite of quartz and felspar*, formed from the oldest rocks, and therefore not entitled to the name of primitive. According to Professor Jameson, this rock rises from below the mica slate. Hence it follows, according to Dr. Macculloch, either that the mica slate is not primitive, or that the primitive are not the oldest rocks.

Rona consists chiefly of rocks of gneiss and hornblende. The gneiss is traversed by numerous veins of graphic granite.

The district of Assynt, forming the western part of Sutherland, contains mountains composed of the same granular quartz as occurs in Jura, and therefore, according to Dr. Macculloch, a mechanical deposite. Associated with this rock, occur compact gneiss, hornblende-slate, and sienitic granite. There occurs also a great deposite of limestone, which appears to alternate with the same rock. It is in two very thick beds, with the granular quartz interposed. This limestone is dark grey, or nearly black, of an earthy aspect, and minute granular texture, and smells offensive when rubbed.

On the 21st of May the rest of Dr. Macculloch's paper on the geology of different parts of Scotland was concluded. The granular quartz of Isla, according to Dr. Macculloch, is a mechanical deposite like that of Jura. It alternates with mica slate and clay slate, and with a very important formation of limestone.

Schehallien, according to Dr. Macculloch, consists of a central ridge of granular quartz, which he considers as a sandstone, flanked on every side by beds of mica slate nearly vertical, and containing subordinate beds of limestone.

The vicinity of Crinan is remarkable for containing nearly vertical beds of grey-wacke and grey-wacke slate.

The rocks bounding the valley of Aberfoyle towards Ben Ledi consist of alternations of grey-wacke and grey-wacke slate, with clay slate; then comes a fine roofing slate, approaching in parts to mica slate, but consisting of grains cemented by mica slate. Beyond this, mica slate makes its appearance.

A paper by the Rev. William Gregor was also read, containing observations on a species of tremolite found in Cornwall. It occurs in a dark green serpentine rock forming the ridge called Clickertor, in the neighbourhood of Liskeard. It was composed of

Silica	62·2
Lime	14·1
Magnesia	12·9
Oxide of iron	5·9
Water	1·0
Manganese and soda	Trace
Loss	3·9
	100·0

On the 4th of June a paper on the Isle of Man, by J. F. Berger, M.D. was read. The length of the Isle of Man from N. E. to S. W. exceeds 30 miles, and its breadth varies from 8 to 15 miles. About 5 miles from the north end a mountainous tract begins, runs parallel to the eastern coast, and forms the small island at the south end of the greater, called the Calf of Man. This belt is divided by three transverse vallies; two in the larger island, and the third forming the strait that separates the one island from the other. The highest mountains are in the northern division; and Sneifeldt, the most elevated, is 2000 feet above the level of the sea. The rocks of which this country is composed belong chiefly to the transition class of Werner. Small-grained granite occurs in one or two places, and at an elevation of not more than 3 or 400 feet above the level of the sea. Gneiss and mica slate are wanting, and so are the oldest members of the clay slate formation. The newest portion of that formation occupies the most elevated parts of the island, where it appears under the form of horn slate and roofing slate. These rocks pass insensibly to the transition class. The oldest transition rock which appears is grey-wacke. The beds of it are mostly less elevated than those of the clay slate; they dip towards the east, and the inclination varies from vertical to about 35°. In this formation occur grey-wacke, grey-wacke slate, and granular quartz slightly micaceous. No organic remains were perceived in any of them.

The preceding formation is covered by a deposite of limestone, less elevated above the level of the sea than the grey-wacke, and approaching more nearly to the horizontal. It consists of beds of shell limestone, resembling those of Kilkenny, Westmoreland, Cumberland, and Durham, together with magnesian limestone, sometimes in beds, but often in patches inclosed within the other. In one or two places the limestone is covered by an unstratified mass of transition amygdaloid; the base of which is a greenish wacke, containing nodules of lamellar calcareous spar, invested by a thin coating of iron pyrites. The only floetz rock that occurs in the island is the oldest sandstone, sometimes so coarse grained as to merit the name of conglomerate, in which case it consists chiefly of fragments of quartz, with a few scraps of decayed slate, and a little iron pyrites. The colour of the sandstone is red, or greyish white. It is more or less slaty, according to the proportion of mica that it contains. It lies unconformably over the grey-wacke, and dips N. W. at an angle varying from 85° to 15°. On the sea shore, and on the slopes of some of the mountains, are abundant blocks of granite, mica slate, and porphyry.

The mines in the island, at Loxey, Foxdale, and Brada Head, are all abandoned. They consist of galena mixed with pyrites, and with the carbonates of lead and of copper. They lie in grey-wacke; but at Foxdale they have been followed into the subjacent granite. The mean temperature of the island is 49·99°.

On the 18th of June a large specimen of nodular agate was exhibited to the Society by Dr. Curry, which he conceived to point out a natural connection between agate and the plasma of the ancients.

Part of a paper, by Mr. Webster, was read, on the fresh water formations of the Isle of Wight, with some observations on the strata lying above the chalk in the south of England. The observations in this paper were partly suggested by the memoir of Cuvier and Brogniart on the strata in the neighbourhood of Paris. Sir Henry Englefield first observed the highly inclined strata of chalk in the Isle of Wight. He entrusted the accurate survey of these curious strata to Mr. Webster. The present paper is the result of his examination.

An elevated ridge of hills runs through the Isle of Wight, in a direction nearly E. and W. from Culver Cliff to the Needles. These hills are composed of strata sometimes nearly vertical, but generally forming an angle with the horizon of from 60° to 80°, dipping northward. The strata consist of the upper and lower beds of chalk; that is, of the chalk with and without flints, covering the chalk marl; and these again are underlayed by calcareous sandstone, with subordinate beds of chert and limestone, clay and carbonized wood. To the north of these strata occur, at Alum Bay, other vertical beds of sand and clay, one of which corresponds in its fossils and other characters with the blue clay containing aptaria, usually known by the name of London clay. Mr. Webster supposes that these vertical beds were originally horizontal, and elevated, by some unknown means, after the formation of the London clay.

If a line in the direction of the central ridge of the Isle of Wight be extended westwards into Dorsetshire, it will be found to coincide nearly with the direction of a ridge running from Handfast Point to Lulworth, and with that already described. It may therefore be considered as a continuation of it. The nearest tract of chalk to the north of this ridge is the South Downs; the strata of which, together with their superimposed beds, up to the London clay, dip gently to the south. Hence the space between may be considered as a great basin or hollow, occasioned probably by the rupture and subsidence of strata originally horizontal. Within this basin, at its southern edge, that is, on the northern coast of the Isle of Wight, occurs a large mass of horizontal strata, in many parts visibly resting on the edges of the elevated strata above-mentioned, and therefore belonging to a period subsequent to that in which the formation of the basin took place. This horizontal deposite differs in its characters from all others hitherto discovered in England, but corresponds with those in the neighbourhood of Paris described by Cuvier and Brogniart. These beds, as they appear in the Isle of Wight, consist of four formations:—1. The lowest fresh water formation. 2. The upper marine formation. 3. The upper fresh water formation. 4. The superficial formation, consisting of an alluvial bed.

The Society adjourned till Friday the 5th of November next.

Article XVI.

List of Patents.

John Mander, chemist, **Aaron Manby**, iron master, and **Joseph Vernon**, furnaceman, of Wolverhampton; for their methods of making the cinder scoria slagg, or by whatever name the refuse produced in the smelting or refining of iron may be called, into forms, which may be used for any purpose to which brick, quarry, tile, slate, or stone, now are, or may be, applied. Dated May 31, 1813. This method has been long practised in the iron furnaces of Sweden. It is probably from that country that the patentees obtained their information.

Thomas Grant, of Biddeford, Devonshire, Esq.; for certain ingredients, by the use and admixture of which with oil, in the preparing and making of paint, a considerable consumption of oil, and also much expense, may be saved. Dated May 31, 1813.

James Oliphant, of Cockspur-street, London, hat manufacturer; for a method of making or manufacturing military caps. Dated May 31, 1813.

Charles Broderip, of Great Portland-street, London, Gentleman; for his improved mode of raising and lowering vessels from one level to another level of navigable waters. Dated May 31, 1813.

Richard Witty, of Kingston-upon-Hull, Gentleman; for additional improvements in or on steam-engines, and tools useful in making certain parts of the same. Dated June 5, 1813.

William Cooke, of Greenwich, in the county of Kent, Esq.; for certain improvements in the art of making and working ploughs of any kind or description. Dated June 15, 1813.

Charles Goodwin, of Finsbury-terrace, London, factor; for an improved socket for a candlestick, consisting of a spring or springs, by which any candle, rush light, or taper, without any paper or other thing being put round it, may be fixed and secured in such socket, and which socket is adapted for the use of any description of candlestick, chandelier, lustre, branch, lamp, or lantern. He has also invented a self-extinguisher, to be fixed to the same, by which the light may be extinguished at any time. Dated June 26, 1813.

ARTICLE XVII.

Scientific Books in hand, or in the Press.

Mr. W. Henley, Member of the London Philosophical Society, is preparing for the Press a series of Chemical Tables, intended to exhibit the properties of all the present known bodies, the result of their union, the composition of the oxides, acids, and their compounds, with the effects produced by the action of heat, light, and electricity; the whole forming a complete abstract of the science of chemistry.

Dr. John Moodie, of Bath, has nearly prepared for publication The Modern Geography of Asia, to be published in two 4to. vols. with an Atlas.

A Fourth Volume of The Antiquities of Athens is preparing for publication.

**** Early Communications for this Department of our Journal will be thankfully received.*

ARTICLE XVIII.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer.			Thermometer.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
6th Mo.										
June 21	N E	30·16	30·14	30·150	66	46	56·0	—		☾
22	N E	30·80	30·14	30·16	67	41	54·0	—		
23	N E	30·10	30·08	30·090	70	55	62·5	—		
24	N E	30·15	30·08	30 115	64	46	55·0	—	—	
25	E	30·18	30·15	30·165	74	50	62·0	·55	4	
26	N E	30·18	30·10	30·140	74	48	61·0	—		
27	E	30·10	29·97	30·035	77	45	61·0	—		
28	Var.	29·97	29 77	29·870	75	53	64·0	—	·37	●
29	S W	29·77	29 64	29·705	74	52	63·0	—	·80	
30	W	29·64	29·54	29·590	65	52	58·5	·40	·75	
7th Mo.										
July 1	N W	29·75	29·64	29·695	70	53	61·5	—	·16	
2	N	29·86	29·75	29·805	67	50	58·5	—	·20	
3	N W	30·11	29·86	29 985	73	42	57·5	·25		
4	N W	30·18	30·11	30·145	70	50	60·0	—	2	
5	W	30·18	30·04	30·110	74	47	60·5	—		☽
6	S W	30·04	29·74	29·890	78	56	67·0	·35		
7	S	29·65	29·57	29·610	78	57	67·5	—		
8	W	29·59	29·55	29·570	75	55	65·0	—	—	
9	W	29·83	29·59	29·710	79	54	66·5	·36		
10	N W	29·91	29 83	29·870	78	51	64·5	—		
11	N W	29·93	29·90	29·915	77	51	64·0	—		
12	W	29·90	29·76	29·830	76	51	63·5	—		
13	S W	29·76	29·63	29·695	76	58	67·0	·38		○
14	S W	29·63	29·60	26·515	71	58	64·5	—		
15	N W	29·70	29·60	29·650	67	50	58·5	—	·60	
16	N W	29·82	29·70	29·760	73	50	61·5	—	·10	
17	N W	29·87	29·82	29·845	74	47	60·5	—		
18	W	29·87	29·73	29·800	73	50	61·5	·43		
19	W	29·73	—	—	73	—	—	—	—	
		30·18	29·54	29·875	79	41	61·69	2·72	3·04	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

◆

REMARKS.

Sixth Month. 21. Brisk wind through the day. 22. Wind more gentle. *Cumulostratus* and *Cirrostratus*. 24. A shower about one p. m. 25. The wind inclines to S.E. Clear twilight, somewhat orange coloured. 27. *Cirrus* changing to *Cirrocumulus* and *Cirrostratus*: twilight somewhat opaque, but coloured. 28. Wind N. E. a. m. The *Stratus* cloud appears to have prevailed in the night. Slight showers at intervals during the day. At 7 p. m. several *Nimbi*, and thunder to the S.W.

which, with occasional lightning, passed by S. to N. E. At 9 p. m. the air was so loaded with vapours as to deposit water on a glass vessel cooled only to 58·5°. It now began to rain heavily, ceasing at ten, with thunder and lightning still in the N. 29. *Cirrus*, *Cirrostratus*, and *Cumulostratus*. About one p. m. a heavy storm of rain and hail, with several electrical discharges. 30. In the forenoon heavy rain, ushered in by a peculiar hollow sound in the wind, then southerly: p. m. wet at intervals. A part of this day's rain was taken by estimate, the guage having been left under cover.

Seventh Month. 3. After an appearance of two distinct orders of cloud during the forenoon, inosculation took place suddenly about one, and the *Cumulostratus*, with a brisk wind, prevailed till sun-set. 4. A slight shower, p. m. From the 5th to the 9th several kinds of cloud prevailed, and occasioned at times considerable indications of rain; of which, however, a few drops only fell, the clouds still passing away to the N. In that quarter on the 9th, evening, we had several distant *Nimbi*, with the usual appearances of a strong electric charge. A single flash of lightning, and some rain, just discernible in the horizon, were the only results. 13. After repeated exhibitions of the *Cumulostratus*, which continued to pass over to the N., we had this night a few drops of rain. 14. Dripping at intervals: the dust laid. 15. A wet day. The vulgar notion that rain on this day (which the Popish Calendar gave to St. Swithin) is followed by the same, daily, for 40 days, if tried at any one station in this part of the island, will be found fallacious. There is, perhaps, in a majority of seasons, a general tendency to rain during this period, which, in Ireland, and on the western coasts of Britain, may, in some, produce the effect in question; and the prejudice hence arising may have travelled into a climate where it does not hold good. 16. Thunder, p. m. during a shower. 17. A slight shower, p. m.: dew on the grass. 18. A fine day: the *Cumulostratus* prevailed, and the evening was very clear, with dew. 19. Showers.

RESULTS.

Prevailing winds, Easterly to the new moon, afterwards Westerly.
Barometer: greatest height ....30·18 inches;
Least ....29·54 inches;
Mean of the period ......29·875 inches.
Thermometer: greatest height ....79°
Least ....41°
Mean of the period.....61·69°
Evaporation, 2·72 inches. Rain, 3·04 inches, in 28 days.

Tottenham, *Seventh Month*, 23, 1813. L. HOWARD.

Errata in our last Number.

Page 37, line 16, *for* "15·00" *read* "13·09."

ANNALS

OF

PHILOSOPHY.

SEPTEMBER, 1813.

ARTICLE I.

Report on a Memoir of M. Berard, relative to the Physical and Chemical Properties of the different Rays which compose the Solar Light. By MM. Berthollet, Chaptal, and Biot.*

M. BERTHOLLET, M. Chaptal, and myself, have been charged with the examination of a memoir lately presented by M. Berard on the physical and chemical properties of the different rays which compose solar light. We proceed, therefore, to give an account of it to the Class.

It is a question long discussed among philosophers and chemists, whether caloric and light be modifications of the same principle, or consist of principles essentially different. Many systems have been constructed in favour of both hypotheses; but the only means of deciding the point is to determine by experiment, and to fix with exactness, the properties belonging essentially to caloric and to light, to ascertain in what respects they agree, and in what they differ; and, finally, to see whether the same principle, always constant in its nature, but acting differently on our organs, and on bodies in different circumstances, be capable of producing all the variety of effect which we observe.

Several skilful philosophers and chemists have already partly directed their attention towards this object. Thus Mariotte discovered that invisible heat radiates like light, and is reflected like it by metallic mirrors, results afterwards confirmed by the experiments of Scheele and Pictet. Mr. Leslie and Count Rumford examined, with particular care, the influence which the nature of the substance, and the state of the surface, has upon the radiation of heat, when it enters into bodies, or escapes from them. Finally, M. Prevost, of Geneva, has included all the phenomena of the radiation of heat in an ingenious theory, which, viewed only in a systematic point of view, as is done by the author, enables him to connect all the phenomena together by laws. Very lately M. Delaroche has added a new fact to these results, which seems to exhibit a gradual passage from heat to light. The rays of invisible heat traverse glass with difficulty at a temperature below that of boiling water; but they traverse it with a facility, always increasing with the temperature, as it approaches the point when bodies become luminous: so that it would appear from these experiments that the modification, whatever it be, which must be impressed upon the invisible rays, to render them more and more capable of passing through glass, makes them approach more and more to the state in which they must be when they penetrate our eye, and occasion the sensation of vision. M. Delaroche has found, likewise, that the rays of heat which have passed through a plate of glass are proportionally more adapted to pass through a second plate. This is a new proof of the peculiar state of these rays, and of the modification which they acquire.

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* Translated from the Annales de Chimie for March 1813, vol. lxxxv. p. 309.

The results which we have stated relate to the motion of heat. Its chemical action, compared with that of light, has been equally studied.

MM. Gay-Lussac and Thenard have proved that all the changes of colour produced by light may be imitated and produced by heat, and by an elevation of temperature not exceeding 212°. Other phenomena previously observed, showed that in that comparison of the actions of heat and light in heating bodies, or producing chemical changes in them, there is a great difference in the rays of different colours. M. Rochon had announced that the heat produced by the different rays of the spectrum was unequal. Dr. Herschell afterwards found that the heating power increases progressively from the violet to the red end of the spectrum. He even fixed the maximum effect beyond the red ray; so that, according to his experiments, the most heating rays of the spectrum were entirely, or nearly, invisible. Dr. Wollaston, and Messrs. Ritter and Beckmann, having examined the opposite, or violet end of the spectrum, found that it likewise possessed peculiar properties; and that there exist, beyond the violet, invisible rays, which possess, in greatest perfection, the power of determining chemical combinations. The experiments of Dr. Herschell, though confirmed by several philosophers, had been called in question by others no less skilful, particularly by Mr. Leslie. On that account it was important to remove every doubt respecting them. It was likewise interesting to know if the invisible or almost invisible rays, situated beyond the extremities of the spectrum, possess any other properties of light. For example, if the reflection of them from polished glass can give them that modification which Malus has distinguished by the name of polarization. M. Berthollet engaged MM. Malus and Berard to undertake this double object. The premature death of Malus has deprived us of the great light which he would without doubt have thrown upon the subject, as he had already done upon other parts of optics by his excellent discoveries; but the researches which he had begun, or projected, have not been lost M. Berard has pursued them with great care. He has finished them with the utmost possible exactness, and has presented the results to the Institute.

M. Berard, with respect to apparatus, has had a great advantage over the philosophers who have preceded him in these researches. He made use of the *heliostate* which Malus had got constructed for the philosophical cabinet of M. Berthollet, and by means of that instrument he obtained a ray of solar light perfectly fixed, on which he could make experiments at pleasure. By decomposing this ray with a prism, he obtained an immoveable coloured spectrum. By placing very sensible thermometers in the spaces occupied by the different colours, he was enabled to compare their calorific effects with the utmost certainty. He ascertained their chemical properties by substituting, in place of the thermometers, chemical compounds easily altered.

He examined, in the first place, the calorific power of the different rays. We know that they are unequal in this respect. M. Rochon, who first observed this inequality, placed the maximum of heat in the yellow ray, where the illuminating power is greatest. Dr. Herschell placed it out of the spectrum, and beyond the red ray. The experiments of Berard have confirmed the experiments of Herschell relative to the progressive heating power of the rays from the violet to the red; but he found the greatest heating power at the extremity of the spectrum itself, and not beyond it. He fixed it at the point, where the bulb of the thermometer was still entirely covered with the red ray; and he found that the thermometer sunk progressively, in proportion as the distance of its bulb from the red ray increased. When he placed the thermometer quite beyond the visible spectrum, in the spot where Herschell fixed the maximum of heat, its elevation above that of the ambient air was only ⅕th of what it had been in the red ray itself. The absolute intensity of the heat produced was likewise less in the experiments of Berard than in those of Herschell. Do these differences depend upon the matter of the prisms and the difference of the apparatus, or on some circumstances depending upon the phenomenon itself? It is not possible for us to determine.

M. Berard wished to know if these properties would exist in each of the pencils into which the ray divides itself in passing through a rhomboid of Iceland spar. He made a ray of solar light pass through a prism formed of a piece of Iceland spar. Each of the two spectra exhibited the same properties. In both, the calorific power diminished from the violet to the red end, and it existed beyond the last visible red rays. Thus, whether this faculty be inherent in the solar rays or not, when these rays are divided by a crystal it goes along with each.

But in this operation the luminous molecules are polarized by the crystal. Do the invisible rays of heat experience the same effect? To determine this point, M. Berard received the solar ray upon a polished and transparent glass, which polarized a portion of it by reflection. This reflected ray was then received upon a second glass, fixed in an apparatus, which permitted it to be turned round the ray under a constant incidence, and this incidence itself was determined in such a manner that in a certain position of the glass the reflection ceased to take place. We know, from the experiments of Malus, that a glass may be always disposed in such a manner that this condition is fulfilled. Things being thus disposed, by collecting with a mirror the calorific and luminous rays reflected from the second glass, and directing them upon a thermometer, M. Berard found, that as long as light was reflected the thermometer was elevated, and of course the heat was reflected likewise; but when, from the position of the second glass, the light was totally transmitted, the heat was transmitted at the same time, and the thermometer was not elevated. In this experiment, then, as well as the preceding, the calorific principle, whatever it may be, never separates from the luminous molecules.

To the ray of solar light employed in this experiment, M. Berard substituted a pencil of radiant heat proceeding from a body hot, but not red, and even not luminous. The effect was the same as before. The thermometer rose when the second glass was so situated as to reflect light, and it did not rise when the second could not reflect light. Therefore the particles of invisible radiant heat are modified by reflection, precisely like light.

After having studied the calorific properties of the different rays of the spectrum, M. Berard examined their chemical properties. When muriate of silver, or other white salts of silver, are exposed to light, they become dark-coloured very speedily. Guaiac thus exposed to light passes from yellow to green, as Dr. Wollaston observed. Gay-Lussac and Thenard discovered another action of this light still more prompt and energetic. When a mixture of oxymuriatic acid gas and hydrogen gas are exposed to the action of solar light, a detonation takes place, and water and muriatic acid are formed. These different phenomena enabled M. Berard to examine the chemical powers of the different rays of the spectrum. By exposing to the different coloured rays, pieces of card impregnated with muriate of silver, or small phials filled with the detonating mixture, he was enabled to judge of the energy of each by the intensity or rapidity of the chemical change which it produced. He found that the chemical intensity was greatest at the violet end of the spectrum, and that it extended, as Ritter and Wollaston had observed, a little beyond that extremity. When he left substances exposed for a certain time to the action of each ray, he observed sensible effects, though with an intensity continually decreasing in the indigo and blue rays. Hence we must consider it as extremely probable, that if he had been able to employ reactives still more sensible, he would have observed analogous effects, but still more feeble, even in the other rays. To show clearly the great disproportion which exists in this respect between the energies of the different rays, M. Berard concentrated, by means of a lens, all that part of the spectrum which extends from the green to the extreme violet; and he concentrated, by means of another lens, all that portion which extends from the green to the extremity of the red. This last pencil formed a white point so brilliant that the eyes were scarcely able to endure it; yet the muriate of silver remained more than two hours exposed to this brilliant light without undergoing any sensible alteration. On the other hand, when exposed to the other pencil, which was much less bright, and less hot, it was blackened in less than six minutes. M. Berard concluded, from this experiment, that the chemical effects produced by light are not solely owing to the heat developed in the body by its combining with the substance of the body; because, on such a supposition, the faculty of producing chemical combinations ought to be greatest in those rays which possess the faculty of heating in the greatest perfection: but perhaps we should find less opposition between these two opinions, if we attended to the different results which may be produced by the same agent placed in different circumstances, and if we considered that agents of a nature quite dissimilar may determine the same combinations when they are employed.

Such is an epitome of the principal facts which M. Berard has established in his memoir. To great accuracy he has united an excellent arrangement in his account of his experiments. He has presented the physical properties of the different rays merely as the results of experiments, the hypothetical causes of which he has abstained from inquiring into: and he has always employed terms so general as to be applicable, whether the properties treated of belong to a substance really distinct and combined with light, or result simply from original differences which exist among the different molecules of the same principle, which, according to differences in the size or the velocity, or in both united, become capable of producing chemical combinations, vision, and heat.

Without attempting to decide between two opinions, which go both beyond the facts observed, we may at least weigh their relative probabilities, and compare the number of hypotheses necessary in each to represent the same number of facts. If we wish to consider solar light as composed of three distinct substances, one of which occasions light, another heat, and the third chemical combinations; it will follow that each of these substances is separable by the prism into an infinity of different modifications, like light itself; since we find, by experiment, that each of the three properties, chemical, colorific, and calorific, is spread, though unequally, over a certain extent of the spectrum. Hence we must suppose, on that hypothesis, that there exist three spectrums one above another; namely, a calorific, a colorific, and a chemical spectrum. We must, likewise, admit that each of the substances which compose the three spectrums, and even each molecule of unequal refrangibility which constitutes these substances, is endowed, like the molecules of visible light, with the property of being polarized by reflection, and of escaping from reflection in the same positions as the luminous molecules, &c.

Instead of this complication of ideas, let us conceive simply, according to the phenomena, that light is composed of a collection of rays unequally refrangible, and of course unequally attracted by bodies. This supposes original differences in their size and velocity, or in their affinities. Why should those rays, which differ already in so many things, produce upon thermometers, or upon our organs, the same sensations of heat or light? Why should they have the same energy to form or separate combinations? Would it not be quite natural that vision should not operate on our eyes, except within certain limits of refrangibility; and that too little or too much refrangibility should render it equally incapable of producing that effect. Perhaps these rays may be visible to other eyes than ours, perhaps they are so to certain animals, which would account for certain actions that appear to us marvellous. In a word, we may conceive the calorific and chemical faculty to vary through the whole length of the spectrum, at the same time with the refrangibility, but according to different functions; so that the calorific faculty is at its minimum at the violet end of the spectrum, and at its maximum at the red end; while, on the other hand, the chemical faculty expressed by another function is at its minimum at the red end, and at its maximum at the violet end, or a little beyond it. This simple supposition, which is only the simple statement of the phenomena, equally agrees with all the facts hitherto observed, and accounts for those established by M. Berard, and even enables us to predict them. In fact, if all the rays, which produce these three orders of phenomena, are rays of light, they must of course be polarized in passing through Iceland crystal, or in being reflected from a polished glass with a determined incidence: and when they have received these modifications, they must be reflected by another glass, if it is properly placed, to exert its reflecting energy on the luminous molecules. On the other hand, if that force is null on the visible luminous molecules, the invisible light will not be any longer reflected: for the cause which occasions or prevents reflection appears to act equally upon all the molecules, whatever their refrangibility may be. It ought, therefore, to act upon the molecules of invisible light, the condition of visibility or invisibility relating merely to our eyes, and not to the nature of the molecules which produce these sensations in us. But though this mode of viewing the facts appears to us the most natural and simple, we cannot but approve the sage reserve of M. Berard in not attempting to decide questions upon which experiment has not yet accurately pronounced.

The Class heard with pleasure the detail of these interesting experiments, which were presented by the author on the same day that he and M. Delaroche obtained the prize offered for the determination of the specific heat of the gases. We propose to the Class to confirm, by its approbation, this new and valuable set of experiments; and we regard it as very worthy to be printed in the *Recueil des Savans Etrangers.*

(Signed) **Berthollet, Chaptal,** and **Biot,**
Commissioners.

ARTICLE II.

On the Daltonian Theory of Definite Proportions in Chemical Compounds. By Thomas Thomson, M.D. F.R.S.

(*Continued from* p. 115.)

I shall continue the table of chemical compounds in this Number a little farther, observing the same method as in the preceding part of this paper.

	Number of atoms.	Weight of an integrant particle.
135. Hydrate of potash....	1 *p* + 1 *w*......	7·132 [a]
136. Hydrate of soda	1 *s* + 1 *w*......	9·014 [b]
137. Hydrate of lime	1 *l* + 1 *w*......	4·752 [c]
138. Hydrate of barytes ..	1 *b* + 1 *w*......	10·863 [d]
139. Hydrate of strontian	1 *st* + 1 *w*......	8·032 [e]
140. Hydrate of magnesia	2 *m* + 1 *w*......	5·868 [f]

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[a] By *hydrate of potash*, I mean caustic potash which has been exposed to a red heat. If we suppose it composed of an integrant particle of potash, and an integrant particle of water, it should consist of 100 potash + 18·867 water. Now Davy, by heating potash and boracic acid together, actually separated between 17 and 18 of water. This I consider as a full confirmation. Berzelius obtained 16 per cent. (*Lärbok i Kemien*, ii. 594;) which is very nearly the exact quantity. It ought to have been 15·872.

[b] This is caustic soda exposed to a red heat. Supposing it a compound of an integrant particle of soda and of water, it ought to consist of 100 soda + 14·362 water, or 84·128 soda + 12·558 water. I do not know that any accurate experiments have been made to determine the proportions of this hydrate.

[c] According to this statement, slacked lime (which is the hydrate) is composed of 100 lime + 31·27 water. Now Lavoisier found it composed of 100 lime + 28·7 water, and Dalton of 100 lime + 33·333 water. The mean of these results gives us 31·016, which very nearly coincides with the number in the table.

[d] By comparing the experiments of Berthollet (*Mem. d'Arcueil*, ii. 42,) with those of Berzelius, (*Ann. de Chim.* lxxviii. 50,) it appears that crystallized barytes exposed to a red heat (hydrate of barytes) is composed of 100 barytes + 12·121 water. If we suppose it a compound of an atom of barytes and an atom of water, its composition will be 100 barytes + 11·632, which almost coincides with experience. Hence the number in the table.

[e] That a hydrate of strontian exists is certain, but no direct experiments have been made upon its composition; but from analogy, it is probable the number in the table is correct. The crystallized hydrate, according to Dr. Hope's experiments, is composed of 1 atom strontian and 13 water.

[f] Hydrate of magnesia is obtained by precipitating magnesia from an acid, by means of potash, and drying it in a gentle heat. It is composed, according to Davy, of 100 magnesia + about

	Number of atoms.	Weight of an integrant particle.
141. Hydrate of alumina..	1 a + 1 w......	4·632 [g]
142. Hydrate of glucina ..	1 g + 1 w......	4·732 [h]
143. Hydrate of yttria....	1 y + 3 w......	11·796 [i]
144. Hydrate of zirconia..	1 z + 1 w......	6·788 [k]
145. Hydrate of silica	1 si + 1 w......	5·198 [l]
146. Hydro-sulphuric acid, or acid of 1·85	1 s + 1 w......	6·132 [m]
147. 2d hydrate of sulphuric acid, or acid of 1·780	1 s + 2 w......	7·264 [n]
148. 3d hydrate of sulphuric acid, or acid of 1·65	1 s + 3 w......	8·396
149. Hydro-nitric acid, or acid of 1·620	2 n + 1 w......	8·888 [o]

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25 water, which agrees nearly with the number in the table. It would not be surprising if a hydrate of magnesia existed consisting of an atom of magnesia united with an atom of water, but incapable of being dried without losing one-half of its water.

[g] Wavellite may be considered as a native hydrate of alumina. If it be composed of 74 alumina and 26 water, it must consist of an atom of alumina + an atom of water. Alumina precipitated from a solution, and dried at 60°, would appear, from Saussure's experiments, to be composed of 1 atom of alumina and 4 atoms of water.

[h] Stated merely from analogy, without any direct experiment.

[i] From the experiments of Klaproth, it appears that yttria precipitated from acids, and dried, is composed 69 yttria + 31 water. Hence the number in the table.

[k] According to Davy, zirconia, when in the state of a hydrate, contains more than $\frac{1}{5}$th of its weight of water. Hence the number in the table must represent its composition.

[l] We have no direct analysis of the hydrate of silica; but the earth is known to absorb about $\frac{1}{4}$th of its weight of water. Hence it must be a compound of 1 atom silica and 1 atom water.

[m] This is the strongest possible sulphuric acid. It is composed of 100 real acid + 22·64 water.

[n] This is the acid which freezes at the highest temperature of all, about 42° Fahrenheit. It is that on which Mr. Keir made his experiments. It is composed, as Dalton has shown, of 100 real acid + 45·28 water. Hence its composition is as stated in the table.

[o] This and the three following hydrates have been ascertained

	Number of atoms.	Weight of an integrant particle.
150. 2d hydrate of nitric acid, or acid of 1·54 ..	1 *n* + 1 *w*	5·010
151. 3d hydrate of nitric acid, or acid of 1·42 ..	1 *n* + 2 *w*	6·142
152. 4th hydrate of nitric acid, or acid of 1·350	1 *n* + 3 *w*	7·274
153. Hydrophosphorous acid	2 *p* + 1 *w*	5·772[p]
154. Hydrate of boracic acid	1 *b* + 3 *w*	9·106[q]
155. Hydrate of peroxide of copper	1 *c* + 1 *w*	11·132[r]
156. Hydrate of black oxide of iron	1 *i* + 1 *w*	9·798[s]
157. Hydrate of red oxide of iron	1 *i* + 1 *w*	10·798[t]
158. Hydrate of deutoxide of tin	1 *t* + 1 *w*	17·837[u]

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by Dalton. The reader may consult his curious table in his *New System of Chemical Philosophy, Part* II. p. 355.

[p] This is the result of the experiments of Sir Humphry Davy; who, by combining phosphorous acid with ammonia, and distilling off the water, found that the hydrate of phosphorous acid is a compound of 2 integrant particles of phosphorous acid and 1 of water. (*Davy's Elements of Chemical Philosophy*, p. 289.)

[q] We shall see afterwards that the weight of an integrant particle of boracic acid is about 5·710. Now Davy found hydrate of boracic acid composed of about 57 acid + 43 water. This approaches the number in the table; but as it does not quite correspond, little confidence can be put in it.

[r] This is the blue substance obtained by precipitating nitrate or sulphate of copper by an alkali. According to Davy, it is composed of 9 peroxide of copper + 1 water. Now 9 : 1 :: 10 : 1·111; and 1·111 is very nearly the weight of an integrant particle of water.

[s] It appears from my experiments that water combines with black oxide of iron. I state the proportions from analogy merely. The increase of weight in my trial was rather less than equivalent to the proportion of water stated in the table.

[t] This is the orange precipitate obtained from oxysulphate of iron by an alkali. It has been ascertained to be a hydrate, but never analysed. I state the proportions merely from analogy.

[u] This is the white powder obtained by precipitating the recent solution of tin in muriatic acid. According to Proust, it is

	Number of atoms.	Weight of an integrant particle.
159. Hydrate of peroxide of tin	$1\,t + 1\,w$	......18·969 [x]
160. Hydrate of deutoxide of nickel	$1\,n + 2\,w$	...... 7·887 [y]
161. Hydrate of deutoxide of cobalt	$1\,c + 1\,w$	......10·458 [z]
162. Hydrate of protoxide of manganese ..	$1\,m + 1\,w$	...... 9·262 [a]
163. Hydrate of oxide of arsenic	$1\,a + 1\,w$	...... 9·132 [b]

These are the only hydrates (not reckoning the salts and vegetable acids) at present known. It is evident, from the slightest inspection of the table, how little they have been examined, and how uncertain we still are respecting the proportions of the constituents of several. They deserve a more careful examination than has hitherto been bestowed upon them; and, if accurately understood, would greatly facilitate many important processes in practical chemistry. It is particularly worthy of inquiry, whether all metallic oxides be capable of forming hydrates, or only some of them. The former is the more probable opinion; but it is very far from being determined.

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composed of 95· black oxide + 5 water. Now this approaches to 1 particle of black oxide and 1 particle of water.

[x] There can be no doubt that this hydrate exists; but it has not hitherto been analysed. I state its composition in the table merely from analogy.

[y] This is the green coloured substance obtained by dissolving nickel in nitric acid, and precipitating by potash. According to Davy, more than one-fourth of its weight is water. This statement approaches most nearly to the supposition that it contains 2 particles of water to 1 of oxide.

[z] This is the blue powder obtained by dissolving cobalt in an acid, and precipitating by an alkali. It has not been analysed. The number in the table is given merely from analogy.

[a] This is the white powder precipitated by alkalies from colourless solutions of manganese. It has never been analysed. Hence the numbers in the table are given only from analogy.

[b] This is the opake white powder, or the small white crystals, of oxide of arsenic. The pure oxide is transparent and colourless, like glass. This hydrate has never been analysed. The numbers in the table are stated merely from analogy.

ARTICLE III.

Contributions towards a Chemical Knowledge of Manganese.

By Dr. John.*

Though manganese was known in the remotest times, and though after Bergman had shown that it contained a peculiar metal, several distinguished chemists occupied themselves with experiments upon it, yet it is one of the metals respecting which our knowledge is most defective. Indeed, we may say that it never has been described in a state of purity. This consideration has induced me to make choice of it as an object of experiment. My labours on it have of necessity been so various, and my observations on it so numerous, that it would fatigue my readers too much were I to publish the whole at once. I shall therefore lay them before the world at different times, and in separate dissertations.

I. *Purifying of the Oxide of Manganese.*

This, which is accomplished with no small difficulty, must occupy us in the first place. That chemists have hitherto attained the means of purifying manganese only imperfectly, is obvious from the many processes proposed, all differing so much from each other.

It is not only to the separation of the iron always contained in the mixture to which we ought to pay attention: copper, also, and not unfrequently lead, together with several earths, are to be found in several of the ores of manganese. In particular, I have found them in the purest grey Saxon manganese.

For the separation of the iron from manganese we do not, it is true, want processes; but I have examined them with accuracy, and have not found any of them singly fully to answer the intended purpose.

Gehlen's well known, and generally practised, method of separating iron from all metals which form a soluble salt with succinic acid, by precipitating the iron from its solution by means of an alkaline succinate, leaves nothing farther to be desired in respect of accuracy; but if we consider the high price of succinic acid, and the continually diminishing quantity of amber from which it is procured, the matter will appear in another light, and induce us to look out for a more convenient method.

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* Translated from Gehlen's Journal fur die Chemie und Physik Dritter Band. Sid. ii. 52.

When I considered the principle on which Gehlen founded his method of separating iron from manganese by means of succinic acid, namely, the insolubility of one of the two resulting compounds, it occurred to me that oxalic acid, or any of its neutral salts, might be used for the same purpose, with more economy.

With this view I mixed a saturated muriatic solution of manganese with some nitric acid, evaporated it to the consistence of a thick syrup, diluted it with much water, saturated the excess of acid with some potash, and left in the solution, for 24 hours, a polished plate of iron, in order to separate the copper which it contained as completely as possible. The solution being now filtrated, I dropped into it a little more potash, to occasion a commencement of precipitation. The solution, thus rendered a little muddy, was treated with a neutral solution of oxalate of potash as long as any iron continued to precipitate. The vessel was then kept for 24 hours in a moderate heat, to enable the precipitate the better to separate from the liquid. The solution contained lead (as was before mentioned). This was easily thrown down by means of sulphate of soda, and the resulting sulphate of lead was reduced upon charcoal by means of the blowpipe.

When the process described in the last paragraph is attentively observed, we find that the peculiar yellowish red or brownish colour which the solution of manganese containing iron or muriatic acid at first has, gradually disappears in proportion as the iron is precipitated. When the liquid is concentrated, indeed, it assumes a peach blossom red colour; but when diluted, it becomes as colourless as water.

When prussiate of soda is dropped into the solution, the oxide of manganese, when no copper happens to be present, is thrown down of a snow-white colour, which does not change, though left exposed to the air for days. Infusion of nutgalls occasions no precipitate. When the solution contains copper, the precipitate is peach blossom red, or crimson red, according to the proportion of that metal present. When the least quantity of any solution containing iron is dropped in, the precipitate has always a shade of blue.

To free the oxide of manganese completely from copper, which is not fully accomplished by the preceding treatment with iron, it is not sufficient to precipitate the oxide with carbonate of ammonia, and to digest the precipitate in liquid ammonia. The affinity between the oxides of copper and manganese is so great that they cannot be separated from each other by means of ammonia. Even after repeating this process four times successively upon the same oxide, I found that it still retained a portion of copper; for prussiate of soda always threw down a reddish precipitate.

When the ammonia is evaporated, it soon acquires a blue colour, in consequence of the copper which it contains. By degrees a bluish white powder falls down, which is a triple compound of carbonic acid, and the oxides of copper and manganese. Carbonate of ammonia dissolves almost nothing of pure carbonate of manganese; but if copper be present, the latter becomes at least partly soluble in the ammonia, but yet in such a manner that a portion of the manganese is dissolved at the same time.

This separation of carbonate of copper-and-manganese during the evaporation of the liquid, though I had previously endeavoured to throw down the whole of the copper by means of an iron plate, gave me considerable trouble.

To obtain a complete separation of the copper from the oxide of manganese, I threw down the whole oxides from the muriatic solution by means of carbonate of ammonia, washed the precipitate well with ammonia and water, and then dissolved it in sulphuric acid. To this solution, previously rendered quite neutral, I added a portion of carbonate of potash, or of ammonia, digested the whole for some time, and then separated the resulting precipitate, which consisted of oxides of copper and manganese, by means of the filter.

In this manner was the copper completely separated, which at first, as I had convinced myself, precipitated in combination with the oxide of manganese. The clear solution freed from the precipitate was completely decomposed by carbonate of ammonia. The precipitate washed and collected was considered as pure carbonate of manganese, proper for chemical experiments; for I had previously ascertained that the ore contained no alumina.

This carbonate of manganese, prepared and purified in the manner above described, I employed in my experiments.

II. *Reduction of the Oxide of Manganese.*

The difficult fusion of this metal, and the property which it has of running into a glass when the oxide is brought in contact with a flux, or when the oxide employed for reduction comes in contact with the sides of the vessel, for a long time prevented chemists from supplying themselves with it in any quantity, and from accurately determining its relation to other bodies, and the proportion of oxygen with which it is combined, either in different native compounds or artificial products.

Another inconveniency which occurs in the reduction of this metal, and which is a consequence of its difficult fusibility, consists in the difficulty of uniting into a button the particles of it after they are reduced into the metallic state.

Gahn's and Bergman's method of performing the reduction of the metal in a crucible lined with charcoal, is, as I have satisfied myself, the only one that succeeds. But in order to be free from the inconveniency of either procuring imperfect grains, or of obtaining them exceedingly small, some contrivances are necessary, with which these chemists were unacquainted, and upon which the success of the experiment entirely depends.

I repeated this difficult experiment more than ten times before I obtained a result not merely accidentally successful, but which constantly turns out the same after the same preparation.

Whether manganese reduced by means of charcoal be in a state, chemically speaking, pure, is a question that remains to be answered; but before I state my doubts on the subject, I shall give a description of the method of reducing manganese, which I found successful.

I lined a Hessian crucible somewhat more than six inches high within, with a paste composed of a small quantity of clay mixed with precipitated silica, a great proportion of charcoal, and the requisite quantity of water. The paste was laid on to the thickness of half an inch every where, except at the bottom, where it was thicker. I now with my hands forced in as much charcoal into the wet mass as the crucible would take up, leaving a conical hole in the middle, dried the crucible for some days in a moderate warmth, and then heated it thoroughly red-hot.

I heated the carbonated oxide, destined for the experiment, during an hour in a covered crucible, in order to drive off the carbonic acid, and rubbed the light brown oxide obtained with oil to a paste. I then destroyed the oil by the application of a moderate heat. This process, the object of which was to mix the oxide intimately with the charcoal of the oil, I repeated once more. At last I rubbed to a fine powder the oxide thus treated with oil, and reduced it with as little oil as possible in a mortar to a firm mass. This I formed into the shape of the cavity in the crucible, into which I introduced it, and filled up all the interval with charcoal powder. The whole was then exposed to a moderate heat for half an hour. A cover was now put upon the crucible; it was placed among burning coals, in order to be heated equably and gradually. Then it was suddenly exposed for an hour and a half to as strong a fire as the crucible could bear without melting.

By this process I obtained from 830 grains of carbonated oxide, 350 grains of the pure metal; so that the loss, as will appear from what follows, was very small; and in experiments of this nature some loss is not to be avoided.

III. *Properties of Metallic Manganese.*

It has a silver white colour inclining to grey; the colour of brittle cast-iron. In the open air it emits a peculiar smell, not very unlike that of rancid fat.

Its lustre is not remarkably great.

Its fracture is uneven, and very fine granular.

It is not so hard as cast-iron, and easily yields to the file.

In point of brittleness it approaches cast-iron.

Mr. G. O. B. R. Karsten and myself found its specific gravity 8·013.

It is not attracted by the magnet; but a very small quantity of iron renders it magnetic.

When the metal is completely free from iron and copper it changes instantly in the air, and therefore cannot be preserved. It becomes immediately yellow and violet, and speedily crumbles into a light brown powder, which soon becomes darker. Even in alcohol, in which I have attempted to preserve it, the pure metal speedily crumbles down to powder. This unpleasant experiment I have made more than once, to my sorrow.

It is best preserved in a vessel completely filled with quicksilver, and covered over with another vessel. It appears, however, that the metal, through length of time, amalgamates with the quicksilver, especially when it is only in small quantity; at least the quicksilver acquires a very thick firm skin. I shall examine this subject more accurately hereafter.

It seems not improper to say a few words in this place about *native manganese*, said to have been found by some mineralogists. Picot la Peyrouse, in the *Memoires de Toulouse*, t. i. p. 256, says, that he found it on the mountain *Rancie*, in the valley of *Viederose*, not far from the village of Sem, in the cidevant county of Foix. Most mineralogists and chemists have combated his assertion, and have grounded their opinion upon the rapidity with which artificially prepared manganese alters when exposed to the air. I am, however, of opinion, that manganese may exist native provided it be in combination with other metals; for I found that when during my experiments a portion of iron (derived from the pitcoal with which I sometimes filled up the crucible) had united itself with the reduced manganese, in such cases the resulting regulus was not altered by exposure to the air, and might be kept in an open vessel without undergoing any farther change. In this state it possessed a small degree of malleability, and was attracted by the magnet.

If the pure manganese be left for some days in contact with the charcoal, it attracts the oxygen of the charcoal (?), and falls down in the state of a brown powder. This ready oxydation at a low temperature makes it necessary, after the reduction of the metal, to break the crucible while still warm, in order as soon as possible to put an end to the action of the charcoal on the metal. This interesting experiment I made, without expecting it, with the loss of the whole of the regulus which I had reduced. I performed the process in the porcelain furnace at Berlin; and because it was necessary to continue the heat of the furnace, I left my crucible in it for two days after the fire was withdrawn.

The metal takes oxygen from the dark brown oxide, and changes it into light brown.

It may be fused with borax without any of it dissolving in the salt. If this experiment is performed in a charcoal crucible, the manganese shows a quite different nature. It requires a much less degree of heat than when the metal and salt are fused alone, and the glass of borax appears to dissolve a very small portion of the metal.

Manganese, when fused with borax, acquires quite the external appearance of tellurium; the colour, the lustre, and the fine granular distinct concretions. These last show a truly regular crystalline structure.

Manganese, fused simply with charcoal, leaves behind it, when it is dissolved in acids, a portion of charcoal, which had been in combination with the metal, as is the case in cast-iron. If the oxide, before its reduction to the metallic state, contained iron, in that case the residuum of charcoal becomes more conspicuous. When the manganese is pure it scarcely amounts to 0·01.

Manganese, fused with borax, undergoes an alteration, when exposed to the air, with still greater facility; and when dissolved in acids, it leaves no charcoal behind it.

IV. *Action of Carbonic Acid on Manganese.*

(A.) *On the metal.*—Carbonic acid exhibits very interesting results on the oxidation of manganese. The same thing undoubtedly would happen with all acids, if their action upon the regenerated oxide could be retarded by means of any medium. It is ascertained that all metals before their solution in acids must be converted into oxides, without which they cannot form metallic salts. Now in very few cases does this oxide become manifest to our senses. The rapid or instantaneous action of the acid upon the oxide makes that impossible, and we conclude only from the gases disengaged (which being either hydrogen or imperfect acids, are bodies that have lost oxygen,) that an oxidation of the metal has taken place.

I will show hereafter that some vegetable acids, which do not act so rapidly upon the regenerated oxide, constitute an exception to this observation. This is the case, in an especial manner, with carbonic acid.

If some metallic manganese in the state of powder be thrown into water impregnated with carbonic acid, and the vessel be left for some days covered up, we shall find the metal, upon examination, changed into a green oxide. If we allow the vessel to continue untouched for some weeks, the oxide (provided the quantity of carbonic acid be sufficient) is changed into white carbonate of manganese; and remains for the greatest part at the bottom of the vessel in that state. Only a small portion of it is dissolved in the water.

Carbonic acid gas acts in the same manner upon the metal. I placed a small porcelain vessel, containing some powdered manganese, in a large receiver, free from water, and filled with carbonic acid gas, and let it remain eight days. The metal was converted into green oxide, and some days after contained white carbonate of manganese mixed with it. This peculiar oxidation of manganese appeared at first truly astonishing. Was the carbonic acid decomposed, and was the oxidation produced by its giving out oxygen to the metal? Not a single drop of water was perceptible in the receiver, to which we could have ascribed the change. Or was it produced by a portion of water mixed with the gas in the state of vapour? The last opinion seems most probable, and will have the greatest number of supporters. But is the first opinion quite improbable? I shall not venture to adopt it till a decisive method of ascertaining when a gas is free from water shall have convinced me that the last opinion is inconsistent with truth.

(B.) *On the brown oxide.*—Manganese, which from exposure to the air has been changed into a light brown oxide, is also altered by carbonic acid, and partly converted into carbonate of manganese. I put the oxide into a small glass, and suspended it for some weeks in a large flask filled with carbonic acid gas. At the end of that time it had assumed a greyish brown colour. Nitric acid disengaged from it carbonic acid gas, dissolved a portion of it, and left another portion in the state of black oxide. From this I conclude that, from the action of the carbonic acid, one portion of the brown oxide had given out oxygen to another portion, and had thus converted it into black oxide, while the portion deprived of a part of its oxygen combined with the carbonic acid.

Remarks on the Carbonate of Manganese.

All solutions of the salts of manganese are precipitated by the alkaline carbonates. When the protoxide of manganese is thrown down in this manner, we obtain a snow white precipitate, which is pure carbonate of manganese, and answers perfectly to that mentioned above.

There is only one combination of carbonic acid and manganese, that, namely, in which the metal is in the state of protoxide. When we endeavour to increase the proportion of oxygen, as, for example, by the application of heat, a portion of the carbonic acid is disengaged, and the whole peroxide produced remains behind when we attempt to dissolve the carbonate in an acid.

Carbonate of manganese, which has a reddish yellow or brown colour, and yet contains no other metal mixed with it, must therefore be considered as a mixture of brown oxide and carbonate of manganese.

When oxymuriatic acid is brought in contact with carbonate of manganese, the carbonic acid separates in the same proportion in which the oxymuriatic acid is decomposed and manganese oxydated. A solution of manganese in muriatic acid, though mixed with abundance of oxymuriatic acid, is precipitated, it is true, by the alkaline carbonates; but the brown or black precipitate in that case consists (obviously at the expense of the oxygen in the oxymuriatic acid) of pure oxide, without the smallest portion of carbonic acid. Neither precipitate renders lime-water in the least muddy when it is treated with an acid in a small Woulfe's bottle, furnished with a bent tube, the extremity of which passes into the lime-water.

Pure carbonate of manganese appears in the state of a soft snow-white powder.

It is quite tasteless, and is not altered in the air at a temperature between 55° and 60°. Hence it may be kept with perfect safety in a close vessel.

I have found that it may with safety be dried in the temperature of 80°, without either attracting more oxygen, or losing any of its carbonic acid. As I very frequently determine the quantity of pure oxide in salts of this nature by the quantity of carbonate, I chose, in order to avoid all inconvenience arising from the presence of a greater or smaller quantity of water in such carbonates, to dry them always at the temperature of 80°. The pure alkalies decompose the carbonate of manganese, combine with the acid, and the white oxide, by exposure to the air, attracts oxygen, and becomes brown.

Carbonate of manganese may be considered as insoluble in water, since eight ounces of that liquid scarcely dissolve half a grain of it. Even water impregnated with carbonic acid gas dissolves it only in very minute quantity, though recently precipitated, and still moist. Eight ounces of water, as strongly impregnated with carbonic acid as possible, dissolved scarcely one grain of carbonate. To choose carbonic acid as a medium to separate manganese in solution from other substances with which it is in combination, as has been proposed by some chemists, is therefore in the highest degree improper. When water containing the greatest possible quantity of carbonate of manganese in solution is exposed to the air, it can hardly be said to cover itself with a skin. This erroneous opinion has probably originated from confounding manganese with magnesia. When a solution of manganese in an acid (the acid being in excess) is decomposed by a carbonated alkali, and the filtered liquid is diluted with water, if it be left exposed to the air, or placed in an artificial warmth, it is soon covered all over with a thin skin; but it is impossible from this to estimate the qualities of carbonate of manganese, or to conclude its great solubility in carbonic acid water; for when this skin is accurately examined, we find that it is a salt composed of carbonic acid, oxide of manganese, the alkali employed in producing the precipitation, and probably of the acid likewise in which the metal was previously dissolved.

In order to precipitate manganese completely from a solution, it is necessary that there should be no excess of acid present. Only the neutral solutions are completely decomposible.

The fat oils dissolve carbonate of manganese by the application of heat, and form with it a compound having the consistence of plaster. This observation was first made by Scheele.

Estimate of the Proportion of the Constituents of Carbonate of Manganese.

I first estimated, by dissolving a hundred grains of carbonate of manganese, the loss of weight sustained by the escape of carbonic acid. By this method I determined the proportion of acid to amount to 34·25 per cent.

To determine the other constituents, I filled a very small good coated retort with carbonate of manganese, of which 120 grains were requisite. The beak of the retort passed into a glass ball, from which proceeded a tube to allow the gas to escape. By the application of a heat, at first gentle, but gradually so high as to make the retort red-hot, the gas and the water were expelled from the salt. After continuing a red heat for two hours, the apparatus was taken to pieces, and the weight of the different products obtained determined. The water in the glass ball weighed 12 grains; and the oxide remaining in the retort, and which had acquired a greenish grey colour, weighed 67 grains.

If we subtract the weight of the oxide and water thus obtained from the 120 grains employed in the experiment, there remains a loss of 41 grains. As the gas which made its escape during the distillation was pure carbonic acid, this loss gives us 41 grains for the amount of that acid in the salt. This agrees very nearly with the result of the first experiment, according to which 100 grains of carbonate of manganese contain 34·25 grains of carbonic acid.

One hundred grains of the salt are composed of the following constituents :—

Protoxide of manganese	55·84
Water	10·00
Carbonic acid	34·16
	100·00

The greenish grey oxide thus obtained dissolves in acids without the extrication of any gas, and the same compound is obtained as would have been got by the solution of the carbonate in the same acid. Hence it is to be looked upon as the real base of the salt; and the white powder, which appears when manganese is precipitated by an alkali, must be considered as a hydrate, or an oxide still combined with a portion of acid.

V. *Action of Sulphuric Acid on Manganese.*

(A.) *On the metal.*—The action of concentrated sulphuric acid upon manganese in the metallic state is very inconsiderable. It produces some heat, and a small portion of the metal is dissolved. Several chemists, it is true, are of opinion that the solution takes place very readily; but I conclude, upon very good grounds, that the regulus which they employed was not quite in the metallic state, but contained some oxygen combined with it.

When the acid is diluted with water, heat is evolved, and hydrogen gas disengaged, which has a very peculiar smell, similar to that of asafœtida. This smell is probably owing to a portion of the metal being carried off in solution in the gas. During the solution the liquid assumes a green colour, which disappears when the acid ceases to act upon the metal. This appearance probably depends upon the formation of green oxide, as when carbonic acid acts upon the metal.

The concentrated solution has a light rose-red colour; and when it is diluted with water, the colour becomes insensible. I have never been able to procure colourless crystals from this colourless solution. Indeed, as the metal which is oxydated at the expense of the water gives at first a coloured liquid, it is not reasonable to expect colourless crystals of sulphate of manganese.

190 grains of concentrated sulphuric acid, of the specific gravity 1·860, required for neutralization 100¾ grains of metallic manganese. The filtered solution (which left ¾ grains of black, brilliant, minutely divided, charcoal,) gave, when decomposed

by carbonate of ammonia,* 205¾ grains of carbonate of manganese dried at the temperature of 80°.

From this experiment it follows, that 100 parts of carbonate of manganese consist of

Metal	48·60
Acid	51·40
Water	
Oxygen	
	100·00

(B.) *On the protoxide.*—Both the pure protoxide and the carbonate of manganese dissolve completely in sulphuric acid, in whatever state of concentration it be. The solution has the same properties as that mentioned in the last paragraph, and what I now say of it may, to save repetition, be applied to both.

When the neutral solution is rapidly evaporated, the sulphate of manganese separates in the state of a granular powder, or embryo crystals; and no crystals are obtained during the cooling of the liquid. To obtain this salt in the state of regular crystals the saturated solution must be left to spontaneous evaporation. I gained the same object more speedily by means of some crystals of sulphate of manganese, which I introduced into a solution concentrated, as far as possible, by rapid evaporation. I then placed the solution for farther evaporation upon the top of a stove, where the temperature was between 68° and 73°. By this contrivance I reduced the whole salt, without exception, to a regular state.

The crystals of sulphate of manganese which first shoot are of a faint rose-red colour. The last crystals have a white colour, and contain a great excess of acid.

Properties of crystallized Sulphate of Manganese.

It crystallizes, 1. In very broad flat four-sided prisms, either complete, or slightly truncated on the alternate edges of the sides. The crystals are usually more or less intermixed with each other, and hence incomplete. 2. In rhombs.

The crystals are completely transparent, have a light rose-red colour, and a bitterish metallic taste.

At the temperature of 55° they remain unaltered in the air, and do not attract more oxygen even when left in contact with oxygen gas. At the same temperature, 2½ parts of water dissolve 1 part of the salt. They are insoluble in alcohol.

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* I always took care to convince myself by repeated experiments that no oxide remained in solution in the liquid.—J.

Their specific gravity amounts to 1·834, that of water being 1·000.

At the temperature of 66° they become opake and white.

When the crystals are heated to redness in a coated retort, they lose their water of crystallization, and a white dry mass remains in the retort, which dissolves in water by the assistance of heat. When the fire is raised so high as to melt the retort, an imperfect decomposition of the salt is obtained. Sulphurous acid is disengaged, and some glaceous sulphuric acid is found in the receiver. There remains in the retort a mixture of sulphate of manganese, and of brown oxide.

The aqueous solution of sulphate of manganese is neither precipitated by oxalate of potash nor borate of soda. Some chemists, indeed, have obtained a precipitate by means of this last salt, consisting, as they say, of borate of manganese; but this only happens when the borate used contains an excess of alkali, as is the case with the borax of commerce. Pure tartaric acid produces no precipitate in this solution. When the liquid into which this acid has been poured is evaporated sufficiently, prismatic crystals make their appearance, which have the properties of sulphate of manganese. Infusion of nutgalls, and the alkaline chromates and arseniates, produce no change upon the solution of sulphate of manganese.

The solution is precipitated by the alkaline prussiates, carbonates, and phosphates, and a white powder is obtained, consisting of the oxide of manganese united with the acid of the salt employed. If the sulphate of manganese contains the smallest portion of copper, the precipitate formed by the action of an alkaline prussiate has a peach-blossom red colour.

Estimate of the Proportion of the Constituents.

One hundred and fifty grains of the salt were dissolved in water, and decomposed, while hot, by means of muriate of manganese. The sulphate of barytes which separated was carefully collected, washed, dried, and heated to redness. It weighed 148¼ grains. As it appears from Klaproth's late experiments that the proportion of acid in dry sulphate of barytes amounts to 34 per cent., it follows that 148¼ grains of the salt contain 50¼ grains of acid.

The solution of manganese thus freed from sulphuric acid was precipitated by means of carbonate of potash. The precipitate, collected, washed, and dried at the temperature of 80°, weighed 83¼ grains, which, from the preceding experiments on the composition of carbonate of manganese, indicate 46¼ grains of protoxide.

As the 53 grains of loss must be imputed to the water ontained in the salt, it follows that 100 parts of sulphate of manganese consist of the following constituents:—

Sulphuric acid	33·66
Protoxide	31·00
Water	35·34
	100·00

(*To be continued.*)

Article IV.

Sketch of the present State of Agriculture in Berwickshire. By the Rev. J. Thomson, Minister of Eccles, in that county.

(*Concluded from* Vol. I. p. 265.)

Fallowing.—In Berwickshire the following mode of fallowing is usually employed. As soon after harvest as possible the land that is to be fallowed receives its first furrow, or ploughing. Care is taken that in clay lands the ridges be sufficiently high, that the water may run off into the furrows which are opened by the plough and spade, that it may thence discharge itself into the ditches which surround the inclosure. This first ploughing is considered as of great utility. It buries the stubble, and thus tends to convert it into vegetable soil. It leaves the field in as dry a state as possible; and the winter frosts pulverize the soil. This ploughing is generally made as deep as possible, because the plough, on account of the softness of the soil, can then easily penetrate to the proper depth. Thus, also, the future ploughings are rendered easier.

The second ploughing is seldom given till the oats be sown, and sometimes also the barley. If the land abound with weeds, such as couch-grass, and the season be dry; then it is well harrowed, and the couch-grass is gathered by the hand, and laid up in heaps. These heaps are afterwards led off the field, and mixed with lime, or lime and stable manure, and thus form an excellent compost. It is of great advantage that couch grass should be gathered as early in the season as possible before it begins to grow, because it can be done both more easily and more completely when the soil has been pulverized by the winter frosts than afterwards: but if the season be wet, the land cannot be sufficiently harrowed, nor the couch grass gathered, till the third ploughing.

The third ploughing takes place when it is supposed all the weeds have vegetated, but not attained such growth as to impoverish the soil. Then the whole is well harrowed, and the couch grass again gathered.

The fourth and fifth ploughings are given after such intervals as may give time for the remaining weeds to show themselves. The harrow again succeeds each ploughing; and the couch-grass, if any remain, is gathered. But if this part of the work has been properly attended to, after the second and third ploughing, few weeds, except annuals, will now remain. More than five ploughings are frequently given if the soil has not been already sufficiently cleaned and pulverized.

It is to be observed, that when the field is very full of large clods, it may be necessary to employ the roller, as clods are often full of weeds. The manure intended for the field, whether lime, or compost, or muck, is put on always at least before the last ploughing; but sometimes the muck is put on before the last two ploughings. We have said nothing of the particular species of ploughing given each time, whether it is gathering, or cleaving, or crossing, because the practice is not uniform; but we may add, that it is customary to plough across the ridges in at least two of the ploughings.

After fallowing in this manner, the wheat is sown. If the season be favourable, that is, not too wet, and the soil very fine, it is thought best to drill the wheat. This is either done with a drill machine, or drill furrows are made with the common plough deprived of its coulter. The wheat is then sown broadcast: it falls chiefly into the drill furrows, and after being harrowed, comes up in regular drills.

Time of Sowing.—Winter wheat is sown from the beginning or middle of September even as late as Martinmas, according to the season, and the convenience of the farmer. After fallowing, from the middle to the end of September is considered as the properest time, when the season is favourable, and the work can be overtaken; after potatoes, as soon as the crop is taken up; after Swedish turnip, about Martinmas. This season (1812) a good deal of wheat was sown in December. After common turnips, as the ground is gradually cleared, it is sown in January, February, and March, when the weather is favourable. It is then called *spring wheat:* and it has been found, by the experience of several judicious farmers, that though the spring wheat be the same as the winter wheat, being the *triticum hybernum,* yet the produce of what has been sown in spring will, when used as seed, ripen a fortnight earlier than the produce of wheat sown in autumn, or before winter.

Sketch of the Turnip Husbandry of Berwickshire.

The introduction of turnips is one of the most important improvements of modern husbandry. No intelligent farmer will deny that the cultivation of turnips was long known and practised in Norfolk before it was adopted in Berwickshire. Some ascribe the first cultivation of turnips in this county to the late Earl of Marchmont; some to Mr John Hunter, who for some time was steward to the same Nobleman; some to Mr. Pringle, of Lees, near Coldstream; but it is generally agreed that Mr. William Dawson, formerly tenant in Frogden, Roxburghshire, who is still alive, had the chief merit of introducing the drill turnip husbandry into this neighbourhood, and of bringing it to its present improved state. To allow this, is to ascribe no small honour to Mr. Dawson: for between the first mode of cultivation, the broadcast; and the present, the drill turnip-husbandry, there is a wide display of ingenuity. The broadcast turnip husbandry could never be carried to any great extent in clay soil, because it did not admit the most simple, and cheap, and systematic method of clearing the soil of weeds. But the drill husbandry, while it ensures at least as good a crop, furnishes an opportunity of cleaning and pulverizing the soil; of introducing the cheapest and most effectual instrument, the plough; and of giving to the hoer a dexterity, a regularity, and a dispatch, truly astonishing. Hence it will be no matter of surprise to any to hear that the broadcast turnip husbandry is universally abandoned, and the drill husbandry firmly established, not only in this, but in all the neighbouring counties.

Preparation of the Field.—The field intended for turnips next season is always ploughed, if possible, after harvest, that it may be finely pulverized by the frost of winter. It receives its second ploughing in the spring, or beginning of summer, after the oats and barley are sown, and the potatoes planted. It is then harrowed, and the weeds are gathered. It undergoes a third, fourth, and fifth ploughing, and as many harrowings, during the months of May and June, and the weeds are repeatedly gathered, as often as it is found requisite. It is also occasionally rolled with a heavy wooden roller, in order to reduce it to powder. Thus it is treated exactly in the same way as fallow.

Ridges.—The most usual mode of forming the surface of land for turnips, is to lay the ridges flat; but in a clay soil, like that of the Merse, raised ridges are preferable.

Drills.—The soil being pulverized, and cleared of weeds, the next thing is to form the whole surface into drills with the plough, at from 26 to 30 inches distance from one another. This distance has been found by experience to be the most convenient, because it affords sufficient room for the sustenance and growth of the turnip, and allows the plough to pass easily between the drills. When the surface is flattened, the field is often drilled obliquely; that is, the drills form acute angles with the former ridges. When the ridges are raised, the drills are sometimes drawn at right angles to the ridges; but the most common mode is to make the drills in the same longitudinal direction as the ridges.

MANURING.—The drills being formed, the dung is next carried to the field, and spread in the furrows. That which is properly rotten is preferred. The drills are then ploughed down over the dung, and new drills formed immediately above the dung.

SOWING.—Turnips are sown with a drill plough, which drops the seed in a straight line on the top of the drill. A small harrow and small roller follow the drill plough, for covering the seed, and pressing it down gently. The seed is sown pretty thick, in order to give the greater certainty of a crop. Some use drill ploughs which sow two, three, four, and even five drills at once: but unless the surface of the drills be perfectly level, such machines do not perform the work so correctly as a single drill plough.

SPECIES SOWN.—The species or varieties of turnips sown are generally the yellow, and white or globe turnip, the root of Scarcity or Ruta Baga, commonly called the Swedish turnip, a plant classed by botanists under a different genus; but as the Ruta Baga is employed by farmers for the same purpose as turnips, they find no occasion for a more scientific classification.

TIME OF SOWING.—The Ruta Baga requiring a greater length of time to bring it to maturity than the turnip, is generally sown before the end of May, when the season is favourable. The yellow turnip is next sown. The best time for sowing the globe turnip is thought to be from the middle of June to the middle of July. The time of sowing must, however, vary, according to the season: and those who require immense fields of turnips must, of course, begin earlier than those who require few.

FIRST HOEING.—If the weather be warm, and the soil neither too dry nor too wet, the seed vegetates in a few days. As soon as the plants have attained sufficient strength and size for weeding and singling, the weeding plough is made to pass along each drill, for cutting the weeds, and pulverizing the soil between the drills. Then the hoers are set to work to single the turnips, to cut the remaining weeds, and pulverize the soil close to the turnips on the narrow space which the plough has not touched. The hoes are made of such length in the iron part as to serve as measures for the interval to be made between every two plants: and by *singling* is meant leaving the turnips single by themselves, and cutting and destroying those on each side. The hoers are generally women and children, accompanied by a steward, or by the farmer himself, to superintend the work. It is wonderful to observe their dexterity, their correctness, and expedition. They push the hoe from them across the drill, destroying all the unnecessary plants which the hoe touches; pull it back with the same effect, seldom leaving more than one plant at every six or eight inches distance. They go on pushing and drawing alternately with the greatest agility and rapidity; so that you would think, the first time you saw a number of experienced hoers at work, that every plant would be destroyed. But when you look over the ground they have passed, you see the plants left single at measured distances, lying on their sides, connected with the ground only by a single fibre. Even then an unexperienced person would think they could never recover themselves. In a few days, however, they are raised erect, by the power of vegetation, and assume a vigorous appearance.

SECOND HOEING.—After the first hoeing the plants make rapid progress. When they have acquired sufficient size for hoeing a second time, the hoers renew their operations. Their business now is to correct any oversights that may have been committed in the first singling; for example, where two or more plants happen to be left together to destroy all but one, and also to cut or kill the weeds thoroughly in the intervals of the drill and on each side. The earth is afterwards drawn up to the roots by a plough. The crop, then left to the influence of heat and moisture, soon spreads its broad leaves over the whole surface of the field, exhibiting the richest verdure to the eye.

CROP HOW EMPLOYED.—When the grass begins to fail, after harvest, the farmer has recourse to his first sown turnips. In order to sow wheat before winter, some take up the Ruta Baga, or Swedish turnips, before Martinmas, cut off the leaves and roots, carry them home, and lay them up under cover, to be kept till the crop of turnips be consumed. Some, also, for the same purpose, take up a part of their common turnips. The common turnip may be preserved for two months, and the Ruta Baga till Whitsuntide. Thus, by means of Ruta Baga and turnips, the farmer is plentifully supplied with green succulent food for his cattle and horses from Martinmas till Whitsuntide.

Growing turnips are often let by the acre to be eaten on the ground by sheep. This may be done without injury to the sheep on a clay soil, immediately after harvest, or in the spring: but in light dry soils may be done through the whole winter. The price per acre is from 5*l.* to 8*l.* in the country; but higher near towns where the sale of milk is extensive. To those, indeed, who have not been accustomed to the taste of turnip milk, as it is called, the milk of cows fed on turnips is at first extremely offensive; but after a little use, the disagreeable taste wears off. Those who cannot reconcile themselves to this taste will find an effectual remedy by dissolving a little saltpetre in the milk when taken warm from the cow.

DIFFICULTY OF REMOVING THE TURNIPS FROM THE FIELD IN WINTER ON A CLAY SOIL.—The grand difficulty is how to carry off the turnips from a clay soil without injuring it; for if carts are made to pass over a clay soil while moistened with rain, the surface is converted into a kind of mortar; and if allowed to remain a given time in this state, it will not produce half a crop of whatever may be sown in it the ensuing season. It is this difficulty which creates the distinction between what is called a turnip soil and a clay soil. From what is called a turnip soil the turnips may be removed throughout the winter without doing any injury.

There is, however, no doubt but that turnips raised on a clay soil are superior in quality and in usefulness to those raised on a gravelly soil; for they are less spungy, possess more specific gravity, and are in general of a larger size than those raised on a light gravel, which, however, is called a turnip soil. Butchers, too, prefer cattle and sheep fed on a clay soil to those fed on a gravelly soil. The only objection to a clay soil for turnips consists in the difficulty of removing them from the field without injuring it by the ruts made by cart-wheels, and the holes made by the horses' feet. If the difficulty could be surmounted, a clay soil would be more valuable. Several ingenious methods are employed for this purpose. The first and most obvious remedy is to make the ridges high. Others not only make the ridges high, but make the drills at right angles to the ridges, that the rain may have an open passage into the furrows between the ridges. Others, when the air is free from frost, pull up as many turnips as upon calculation will suffice them for a few weeks, cut off the leaves, lay the turnips in small heaps, and cover them with the leaves to preserve them from the effects of frost and wet till the frost consolidates the ground to cart them off. Turnips may be preserved two months in this way. It is a rule of great consequence to plough the ground as soon as possible after the turnips are removed, if in a proper state, that is, if not too wet. When the ridges are pretty high the ground will be frequently in this state during the winter.

Article V.

Remarks on the Measurement of Minute Particles, especially those of the Blood and of Pus. From Dr. Young's Medical Literature, 8vo. Lond. 1813, p. 545.

(Concluded from p. 122.)

V. *Microscopical Fallacies.*

I shall here take the liberty of inserting some remarks, which I cannot attempt at present to render intelligible to any, who have not entered into the minutest refinements of physical optics: to such as are unacquainted with the latest investigations, I fear they must appear involved in a degree of obscurity almost enigmatical.

When a small object is viewed in a microscope, especially if the light is admitted by a limited aperture, it will often appear to be surrounded by some lines of light and shade, or of colours, which might be supposed to depend on its magnitude, in the same way that the eriometrical colours are derived from the magnitude of the objects examined. In reality, however, their existence and their dimensions depend on the aperture of the microscope, and not on the magnitude of the particles in its focus. To prove that this aperture may produce such an effect, hold any object, for instance, the finger or the nail, so as to intercept all the light of a candle, except a narrow line, and this line will seem to project other lines parallel to it into the adjoining shade. Now these lines depend on the interposed object on one side, and on the margin of the pupil on the other: for if we take an object a little narrower than the pupil, we may see them on both sides of it; and causing the pupil to contract by throwing more light on the opposite eye, they will expand, as the space, through which they are admitted, is diminished by the contraction. We may also very distinctly observe, if we look in this manner at a narrow line of light instead of a candle, that the dispersive powers of the eye manifestly convert its image on the retina into a spectrum of red, green, and blue light: sufficiently confuting the conjectural hypothesis of the achromatic property of its refractive substances. If again we substitute a minute hole or slit in a card for the interposed object, the sides of this aperture will now determine the magnitude of the fringes which are seen at the edge of the candle, and their dimensions will be no longer variable, whatever may be the state of the pupil. But the candle must in this case either be placed at a distance, or be partly concealed from the eye, unless one edge of the aperture project so far beyond the other, as to limit its visible extent. Now the substance, in which the lens of a microscope is contained, presents a small aperture capable of exhibiting effects of this kind, which, however, can only be expected to appear when the light is peculiarly circumstanced. The aperture of the highest magnifier that I have employed is $\frac{1}{60}$ of an inch, which answers to about n. 330 of the scale of the eriometer, and would consequently exhibit a bright ring at $\frac{1}{330}$ of the distance of a minute object viewed through it, while the darkest part within this ring would be at about $\frac{2}{3}$ of that distance: and the focal distance of the lens being about $\frac{1}{45}$ of an inch, the diameter of the apparent dark circle would be $\frac{1}{11140}$ of an inch, and that of the bright one $\frac{1}{7425}$; and the dimensions would be nearly the same if any other small lens were employed, with an aperture half as great as its focal distance; so that the constancy of such an appearance, notwithstanding a change of magnifiers, might increase the probability of error. It is obvious that a shade of this kind, surrounding the central parts of a globule, if they happened to be much brighter than the rest, might give rise to a mistaken idea of inequalities in its form or structure; and it is possible that when a particle is darker than the surrounding medium, some parts of its surface may have lines of a similar nature projected on them in an inverse order. The particles of the blood are about $\frac{1}{5000}$ of an inch in diameter, varying from $\frac{1}{6000}$ to $\frac{1}{4000}$; and it is extremely possible that an object of these dimensions may exhibit a light point near its centre, which may be surrounded by a dark and then by a light annular shade within the limits of its disc. There are also several other sources of error in different lights, and in a focus more or less imperfectly adjusted; it is, however, sufficiently evident that no fallacy of this kind can have given rise to all the appearances, which have been already described, as observable in the particles of the human blood, and still less to those which are observable in the blood of some other animals.

VI. *Changeable Colours.*

In examining some of the dust of the lycoperdon, I had put it with a drop of water on a glass, when I observed a purple tinge in the water, which I thought at first was a stain extracted from the powder; but the water viewed separately was perfectly transparent, and the light transmitted directly through the water, when the globules were present, was of a yellowish green. After some consideration, I conjectured that this appearance of colour must be analogous to that of the mixed plates which I had formerly observed, depending on the difference of refractive density of the water and the globules, (Nat. Phil. I. 470, Pl. 30, F. 430, II. 633.) and by substituting fluids of different densities for water, I had the pleasure of finding my conjecture confirmed; for when the water was saturated with salt, the yellow green became nearly blue, and the purple redder or browner; and when olive oil was employed, the light directly transmitted was purple, and the oblique light greenish; in balsam of Tolu again, this purple became red, and the indirect light afforded a faint blue. In air too, I found that the powder appeared of a bright blue green by direct light, and of a purplish hue with a light a little oblique; but when the obliquity became a little greater, the tint changed to a brownish yellow green, which continued afterwards unchanged; this alteration may perhaps be derived from the admixture of a portion of light coming round the particles by a more circuitous route. By comparing the opposite effects of water and olive oil, of the refractive densities 1·336 and 1·379, the refractive density of the particles themselves may be calculated to be 1·62, or somewhat less.

Grey beaver wool seems of a purplish hue in direct, and greenish in oblique light, both in air and in olive oil; its grey colour seems to be derived from a mixture of these tints; in olive oil, the rings of colours which it affords are considerably altered in their appearance, the reds becoming every where very faint. Lead precipitated from its acetate, or silver from its nitrate, by common water, affords a reddish direct and a bluish indirect light, and the same seems to be true of smoke, and of other bodies consisting of very minute particles: but when the indirect light is very powerful, smoke sometimes appears reddish in it, as might be expected from a collection of very small opaque instead of transparent particles.

Mr. Delaval has observed that an infusion of sap green appears of a bright red by transmitted light, and the case seems perfectly analogous to that of the dust of the lycoperdon: the green becoming somewhat yellower, when the gum, with which the colouring particles are mixed, is diluted with water. But this is not the universal cause of a difference of colours exhibited by pigments in different lights; the carthamus, or pink dye commonly sold for domestic use, affords an unequivocal instance of a substance exhibiting colours analogous to those of thin plates, which have been adduced by Newton, in illustration of the colours of natural bodies; the reflected light being undeniably of a yellow green, while the transmitted light is of a bright pink colour. Here the light regularly reflected from the surface only, especially when dry, gives the colour opposite to that of the transmitted light; all the light passing through the fluid, even indirectly, giving a pink colour. But the infusion of the lignum nephriticum seems to hold a middle place between this substance and those which have been mentioned before; the dry extract is of a brownish yellow only; an infusion, not too strong, gives the same colour, verging to orange, by direct transmitted light, and a bright blue by light reflected, or obliquely dispersed within the infusion, or at its surface. The solution of the carthamus affords no green reflection from its surface, and varies in its hue, in different lights, only from crimson to scarlet. The tinging particles of the lignum nephriticum, like those of the precipitated lead and silver, are probably extremely minute, since the colour is but little changed by changing the density of the fluid. It often happens that a blue colour, precisely like that of this infusion, is reflected by green glass bottles, which, when seen by transmitted light, exhibit only a reddish brown colour. The inner bark of the ash is also said to have a property similar to that of the lignum nephriticum. The particles of the blood do not derive their colour from any of the causes which have been mentioned, since it may be extracted from them in a clear solution.

When I attempted to explain the colours of mixed plates, which I had produced by partially moistening two lenses very slightly convex, I observed that the reflection of the light from the internal surface of a denser medium must be supposed to invert its properties with respect to the production of colours by interference, as is naturally to be supposed on the principles of the undulatory theory. But when the obliquity is so considerable, it is not very easy to assign a reason for this inversion; and the experiments, which I have now mentioned, make it necessary to assume a law, which I cannot explain, that every very oblique reflection inverts the properties of light with respect to interference. This conclusion confirms the assertion of Newton, that a dark space, bordered by light, will appear in the centre of a portion of light transmitted between the edges of two knives placed very near each other, and the opinion of Mr. Jordan, that the augmentation of a shadow by diffraction is to be considered as the first dark space belonging to the coloured fringes. I had obtained a different result in an experiment similar to Newton's, because I was not aware of the necessity of employing very sharp edges; for when the edges are blunt, the light is reflected from the one to the other in such a manner, as wholly to destroy the appearance of a central dark space; but in any case this source of error may be avoided, by causing one of the edges to advance a very little before the plane of the other, so that half of the fringes may disappear. It is, however, necessary to suppose this inversion confined to cases of extremely oblique reflection, for when the deviation of the light from a rectilinear path becomes a little more considerable, its effects are no longer perceptible; the second and third fringes scarcely ever requiring any material corrections of the calculations from which it is excluded. The same inversion must also be attributed to the light bent by diffraction round the remoter side of a fibre: for this light always co-operates in the first instance with that which is reflected from the nearer side. The extent of the central white light is indeed so great, that all the coloured appearances may almost be considered as beginning at such a distance, that the first dark space is exactly where the simple calculation would lead us to expect the white; since the value of the unit of the eriometer ought to be, according to this calculation, about $\frac{1}{44000}$ of an inch, instead of $\frac{1}{30000}$; and indeed this value agrees very accurately with experiment, where the two portions of light concerned are exactly in similar circumstances; as may be observed in some of the parallel lines drawn on glass in Mr. Coventry's micrometers, probably where they happen to be single, for in general they are double, and exhibit colours corresponding to an interval much smaller than their regular distance: but in some parts we may observe colours exactly corresponding to their distance, for instance, to $\frac{1}{500}$ of an inch, according to the simple principle of considering each unit as equal to about the 43000th of an inch. Hence it seems that the necessity of a correction depends on the different state of the lights reflected from one side of a fibre, and diffracted round its opposite side, and that when they proceed in a similar manner from two neighbouring parallel lines, the necessity no longer exists. What may be the cause of this irregularity, will perhaps be understood when we understand the cause of the singular phenomena of oblique reflection discovered by Mr. Malus, and we have no reason to expect to understand it before.

VII. *Glories.*

I have had an opportunity of ascertaining, that the clouds which exhibit the white and coloured circles, sometimes denominated glories, are certainly not composed of icy particles; and I have succeeded in deducing an explanation of these phenomena from the same laws, which are capable of being applied to so many other cases of physical optics. In the theory of supernumerary rainbows, (Nat. Phil. I. 471, Pl. 30, Fig. 451, II. 643,) I have observed that the breadth of each bow must be the greater as the drops which afford it are smaller; and by considering the coloured figure, in which their production is analysed, it will be obvious, that if we suppose the coloured stripes extremely broad, they will coincide in such a manner in one part as to form a white bow: the red, which projects beyond the rest, being always broadest, so that if all the stripes be supposed to expand, while they preserve their comparative magnitude, the middle of the red may coincide with the middle of the blue; and it will appear on calculation that a white bow will be formed, a few degrees within the usual place of the coloured bow, when the drops are about $\frac{1}{3000}$ or $\frac{1}{4000}$ of an inch in diameter. It is remarkable that in such cases the original rainbow is altogether wanting, and probably for a similar reason, we scarcely ever see a rainbow in a cloud which does not consist of drops so large as to be actually falling, although I have once seen such a rainbow ending abruptly at the bottom of a cloud: it may be conjectured that the edge of the light is in such cases so much weakened by diffraction, that it is too faint to exhibit the effects occasioned by a larger drop. Dr. Smith has made a remark somewhat similar, (Opt. r. 501,) which, if not completely satisfactory upon the principles which have been mentioned, is certainly altogether unintelligible upon his own.

The coloured circles, immediately surrounding the shadows of the observers, may be deduced from the effect of the same minute particles of water, upon the light which has been four, and perhaps five, times reflected within the drops, which may, after transmission, coincide in direction with another portion, passing on the opposite side of the centre; and the drops about $\frac{1}{3000}$ or $\frac{1}{4000}$ of an inch in diameter would in this manner produce a faint corona, of such magnitude, that the limit of green and red, employed in the use of the eriometer, should be at the distance of about five degrees from the centre of the shadow; which, as nearly as I could estimate it, was its real distance in the appearance that I observed.

Article VI.

General Views of the Composition of Animal Fluids. By J. Berzelius, M.D. Professor of Chemistry in the College of Medicine at Stockholm.

(*Continued from* p. 26.)

Inquiry into the Influence of the Iron contained in the Colouring Matter, in producing its Colour.

The greatest chemical difference that is found between fibrin, albumen, and colouring matter, consists in a quantity of oxide of iron being contained in the ashes of the colouring matter, while none, or at least an infinitely small portion, is afforded by the others. Parmentier and Deyeux, to whom we are indebted for an elaborate memoir on the blood, have conjectured that the iron, contained in the blood, was dissolved in a way analogous to the alkaline tincture of Stahl, an opinion which has since been controverted by Fourcroy and Vauquelin, who, on their side, have endeavoured to prove that the colouring matter of the blood was a solution of red subphosphate of iron in albumen. We shall find that neither of these theories can be true, and that the mode in which the iron is combined with the colouring matter will probably long remain unknown.

The colouring matter dried, and exposed to fire in an open crucible, melts and swells up, and at last burns with flame. It leaves behind a porous coal, which cannot be incinerated without the greatest difficulty. For this purpose it must be reduced to a very fine powder, and exposed to the fire in very thin layers. While the charcoal is consuming, it continually exhales a smell of carbonate of ammonia, which proves that the constituents of ammonia are not disengaged from the charcoal by heat alone, but that the influence of oxygen is also required in order to effect the separation. The ashes remaining after the destruction of the carbon are yellow and pulverulent.

The disengagement of ammonia from a mass of burning charcoal which has already been exposed for a long time to fire, is undoubtedly a very remarkable phenomenon; but another and a no less singular fact is presented by the same substance. If the charcoal which has already been long burning, and whose surface is covered with yellow ashes, be pulverized and boiled in nitromuriatic acid, the acid dissolves the ashes already formed, but does not deprive the remainder of the charcoal of the property of affording a fresh quantity of ashes on re-exposure to the fire. These observations seem to prove that the carbonaceous matter of the colouring substance which remains after the flame has ceased to appear, and after the whole mass has been subjected to a strong red heat, cannot be, as was before supposed, a mechanical mixture of charcoal with the phosphates and carbonates of the earths and of iron. We must therefore consider it as a chemical compound of carbon, phosphorus, sulphur, with calcium, ammonium, and iron; and it appears that it is in a mode analogous to this combination that the iron as well as the calcium, phosphorus, &c. are united with the charcoal, and other constituents of the colouring matter; for it is very evident that the mode in which combustible bodies combine with one another, and with a small portion of oxygen in organic substances, is totally different from that which prevails among the inorganic productions of nature.

To return to the ashes of the colouring matter; water extracts from it a very small quantity of soda mixed with muriate of soda. I have great reason to suspect that these two constituents are merely accidental, and that they appear in consequence of the impossibility of freeing the crassamentum entirely from serum. If the alkaline liquor which is obtained from the ashes, be saturated by acetic acid, evaporated to dryness, and again dissolved, a precipitate will appear on the addition of lime-water, which is phosphate of lime, but in so minute a quantity as not to be appreciable.

I incinerated 20 grammes (400 grains) of colouring matter, till the charcoal was completely destroyed, and obtained 0·25 grammes (5 grains) of ashes having a yellowish red colour. By an accurate analysis, I found it composed of

Oxide of iron	50·0
Subphosphate of iron	7·5
Phosphate of lime with a small quantity of magnesia	6·0
Pure lime	20·0
Carbonic acid, and loss	16·5
	100·0

But this phosphate of iron was not, in all probability, contained in the colouring matter, not even in the ashes: it has evidently been a product of the analytic process. It is produced in like manner when oxide of iron and phosphate of lime are dissolved together in an acid, and afterwards precipitated with caustic ammonia. Part of the lime remains in solution, and the iron seizes upon its phosphoric acid.

But as one mode of argument will hardly suffice to overturn a theory maintained by chemists of such eminence as Fourcroy and Vauquelin, and for the support of which they have brought forward many positive facts, I have made a number of experiments with a view of throwing light on this subject, and have not met with a single one, which did not appear in contradiction to the opinion of these celebrated analysts. Some of these experiments I shall now relate.

(A.) The prussiates, as we have already seen, produce no effect on the colouring matter of the blood; and yet they detect, after 24 hours, the least quantity of any ferrugineous salt added to it, having the red oxide of iron for its basis.—(B.) A watery solution of colouring matter, mixed with gallic acid, acquires a beautiful red colour, but the acid produces no precipitate. By adding to the solution of colouring matter one or two drops of a dilute solution of tannin, the liquor becomes of a beautiful red, without any precipitate appearing. But if, on the contrary, the solution of tannin be concentrated, it precipitates the colouring matter, and gives it a pale red colour. None of these effects seem to prove the presence of a salt with the base of oxide of iron.—(C.) The aqueous solution of colouring matter, mixed with solution of barytes, is not precipitated: at the end of 24 hours a small quantity of phosphate of barytes is found at the bottom of the vessel, and the liquor has assumed a green colour by the action of the alkaline base. Lime-water produces no change.—(D.) A solution of sulphuret of potash produces no change in the solution of colouring matter, except that the red colour is slowly converted into green by the action of the alkali. A drop of acetic acid precipitates the sulphur of a white colour. (E.) It is well known that none of the mineral acids deprive the colouring matter of its colour, which ought to be the case if that colour had been owing to the presence of the subphosphate of iron. The phosphoric acid mixed with the colouring matter blackens without precipitating it: whereas, if the opinion of the French chemists were correct, it ought to produce a neutral and colourless phosphate.—(F.) A drop of a solution of acetate of lead exalts the colour of the colouring matter, and a larger quantity throws down a fine red precipitate.—(G.) The nitrates of silver, of lead, and of mercury, blacken the colouring matter, and when added in larger quantity precipitate it.

These experiments prove that the iron in the colouring matter is not contained in it in such a way as to admit of being detected by the best re-agents we possess, until the composition of the colouring matter is totally destroyed.

But how can these facts be reconciled with the following, mentioned by Fourcroy in his *Système des Connoissances Chymiques?* "Vauquelin and I," says he, "have found that the subphosphate of oxide of iron is easily dissolved in albumen by a slight agitation, and without the aid of heat, and that this solution possesses a bright red colour, similar to that of the blood. This colour becomes still more vivid by adding a little caustic alkali, which facilitates still more the solution of the subphosphate in albumen."

It was not till after having made and carefully repeated many experiments, that I could venture to pronounce this opinion to be void of foundation. I shall not seek to form any conjecture with regard to the circumstances which may have deceived these distinguished chemists, but shall merely relate my own experiments on the subject.

The subphosphate in question, newly prepared and still moist, easily mixes with serum, giving it the colour of rust; but it subsides in process of time, and may be entirely separated from it by means of a filter. The neutral phosphate of oxide of iron, mixed with serum also, does not dissolve, and the addition of caustic alkali instantly produces the subphosphate; but this last remains undissolved, and communicates to the liquor a colour of rust, which is quite dissimilar to that of the colouring matter of blood. The serum deprived of subphosphate by filtration retains a pale yellowish colour, which is owing to a small quantity of oxide of iron held in solution.

If serum is mixed with the subphosphate in question, and phosphoric acid is added, so as to dissolve the subphosphate, a clear rust-coloured liquor is formed, from which a small addition of caustic alkali precipitates a little albumen, which is again dissolved by a slight excess of the alkali; and then the solution loses its red colour, and the subphosphate falls down, and may be collected on the filter.

In all these experiments the albumen dissolves the oxide of iron, even in greater quantity than exists in the colouring matter of the blood; but this solution is yellowish, and has but little body of colour; and the oxide of iron is shown by the usual reagents. The albumen of serum will also dissolve many other metallic oxides; for instance, that of copper, as Vauquelin has long since proved; and it is doubtless the albumen which is the menstruum of the oxide of mercury, found dissolved in the blood during a course of this metal for the cure of syphilis. The oxidulum (or black oxide) dissolves easily, and still more copiously, in serum, forming a perfectly limpid sea-green liquor. The solution is readily obtained by adding to serum some salt of iron, which has the black oxide for its base, and neutralizing its acid by an alkali. When this green solution is exposed to the air, it absorbs oxygen, deposits red oxide of iron, and becomes yellow. The compounds of oxide of iron and albumen are decomposed by the mineral acids, which precipitate the albumen colourless, and retain the iron in solution. The prussiates alone do not disturb the solution of iron in albumen, because the metallic oxide is not here dissolved by any acid; but if, after mixing them, a little muriatic acid is added, an exquisitely beautiful azure blue precipitate appears, consisting of albumen and Prussian blue. If the acid phosphate of iron is dissolved in acetic acid, and afterwards serum be added, followed by a little caustic alkali, the albumen and subphosphate are precipitated together, of a rust-colour, which is not changed by drying, but has no resemblance to the colouring matter of blood. In a word, I have not been able to find any method of combining albumen with subphosphate of iron, or with any other salt of this metal, so as to produce a compound identical with the colouring matter of the blood.

But from the result of all these experiments, what appears to be the difference between venous and arterial blood? This question I am unable to answer; nor can I explain the difference between these substances before and after coagulation.

I have already mentioned that the liquid, out of which the colouring matter has been coagulated by heat, has at first a tint of red, which it loses by cooling; at which time it further deposits a small quantity of colouring matter. This liquid, whilst evaporating, becomes green, owing to the action of the uncombined alkali on the small portion of colouring matter that it still retains. This liquid holds also in solution all the salts, and the soluble animal substances belonging to that portion of the serum which still adheres to the coagulum, and cannot be entirely separated from it. The coagulum I have found, by one analysis, to consist (exclusive of the saline and uncoagulable ingredients) of 64 parts of colouring matter, and 36 parts of an insoluble mixture of fibrin and albumen.

Of the Serum, Albumen, and Salts of the Blood.

When serum is heated in a glass vessel, over a water-bath, it solidifies and forms a pearl-coloured jelly, transparent at the edges. If it is stirred the coagulation is more uniform. It has been said to blacken any silver instrument employed to stir it; but this only happens when the serum has begun to putrefy, or when the bottom of the coagulum has been burnt. As this blackening of silver is owing to sulphur, this substance has also been reckoned among the constituent parts of blood. But it would be equally proper to consider carbon and hydrogen as constituent parts, since these enter into the composition of albumen, in the same way as sulphur does.

Muriatic acid coagulates serum. When heated, a small quantity of azotic gas is evolved. This coagulum has exactly the same properties as the compound of fibrin and muriatic acid.

The sulphuric and nitric acids also produce with the albumen of serum precisely the same compounds as with fibrin.

Phosphoric acid does not coagulate serum.

The acetic acid does not coagulate serum, and when this acid is in sufficient quantity, it prevents the coagulation on heating.

In short, the albumen of serum produces exactly the same compounds with acids and alkalies as the fibrin does, and therefore to avoid repetition, I shall refer the reader back to my observations on this latter substance. The action of alcohol also is perfectly similar in both cases.

There appears therefore to be very little difference between fibrin and albumen; and the latter seems to be intermediate between fibrin and the colouring matter. The only character of distinction between fibrin and albumen is, that albumen does not coagulate spontaneously, but requires a higher temperature for that purpose. Coagulated albumen does indeed dissolve more slowly than fibrin or colouring matter in acetic acid and in ammonia, but probably this is owing to the influence of the heat employed for its coagulation.

Exper. 1. 1000 parts of serum evaporated to perfect dryness, (that is to say, so as easily to be reduced to powder,) left 95 parts of a yellowish, semi-transparent mass, resembling amber, that split to pieces in drying, which, in curling up, carried with them thick scales of the porcelain glazing of the evaporating vessel.

2. I digested 10 grammes of the dried powder in cold water. The albuminous portion softened, and became gelatinous. I separated by the filter the liquid from the insoluble part, and washed the latter repeatedly with boiling water. The undissolved albumen dried in the filter, weighed 6·47 grammes, and did not give up its earthy phosphate, by subsequent digestion in muriatic acid, as this acid remained clear on saturation with an alkali.

3. The solution which had passed the filter was evaporated to dryness, during which thick membranes formed at the surface, and the solution gelatinized before it was perfectly dry. I digested this residue in alcohol, whilst it was still gelatinous; the spirit assumed a yellow colour, and on evaporation left an alkaline yellowish deliquescent mass, weighing 0·92 grammes. This consisted of soda, holding albumen in solution, of muriate of soda, and muriate of potash, of lactate of soda,* and of an animal matter which always accompanies the lactate.

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* In mentioning the *lactate of soda* in this place, I wish to make some observations on the lactic acid, as one of the constituent parts of all animal fluids. It is well known that this acid was discovered by my illustrious countryman, Scheele. Latterly, the French chemists have examined this acid; and Fourcroy, Vauquelin, Thenard, and Bouillon La Grange, have sought to prove that Scheele was mistaken in the supposed peculiar nature of this acid, and that it is only a combination of acetous acid with some animal matter.

These chemists, however, have made no attempts to obtain this animal matter separate from the acid, and have never succeeded in producing the lactic acid by means of the acetic; but this is the proof which they give of Scheele being in an error. Lactic acid is combined with an alkali, the resulting lactate is distilled with concentrated sulphuric acid, and in the receiver is obtained a mixture of sulphurous, muriatic, and empyreumatic acetic acid, the latter of which is purified; and hence it is, that we are informed that the pretended lactic acid is only the acetous united with some animal matter. But it appears to me, that the French chemists have only cut the Gordian knot; for one of the properties of the lactic acid is to be incapable of volatilization, and it is a property of the sulphuric acid to change many organic substances with which it is distilled, into the empyreumatic acetous and sulphurous acids. By a parity of reasoning, almost every one of the vegetable acids might be proved to be only acetic acid, combined with some matter which deprives it of its volatility, without destroying its other acid properties; and in fact it is thus, that Bouillon La Grange has inferred that the malic and gallic acids are only varieties of the acetic. In an analysis of muscular flesh, which I made in the year 1806, I found that the humours of the muscles contained a free acid, which, by many experiments, I discovered to have all the properties which Scheele attributes to the lactic acid. I collected a quantity large enough for examination, and I succeeded by different methods, in obtaining it in greater purity than Scheele had procured his. I examined a great number of its saline combinations, with alkalies, earths, and metallic oxides, the particulars of which are given at length in the second volume of my *Treatise on Animal Chemistry, Stockholm*, 1808, p. 430, & seq. If, therefore, it is allowed, that two acids that produce saline compounds different from each other cannot be identical in their nature, the distinction between the lactic and the acetic acids cannot be controverted. It is since that time that I have discovered the lactic acid, free or combined, in all animal fluids.

Many chemists have observed, in their analysis of animal fluids, that the alcoholic solution, when evaporated, leaves a yellow deliquescent extractive mass. Of late, more attention has been paid to this extract, and if I mistake

The portion not dissolved by alcohol, and digested with water, left a fresh residue of albumen, weighing 1·95 grammes, and possessing the same characters as that of *Exper.* 2. The watery solution could not be made to gelatinize, and did not hold the smallest quantity of gelatine. Besides the alkali, it contained an animal matter easily precipitable by tannin and by muriate of mercury, and which appeared to me to be extracted from the albumen by the boiling water, during its coagulation, and to be analogous to the substance obtained by boiling fibrin with water.

We have been told, that the blood contains much alkaline and earthy phosphates. I coagulated a large quantity of serum, and thus procured a good deal of the residuary uncoagulable fluid. This I mixed with barytic water, which after a time gave a slight precipitate, soluble in muriatic acid. Some of the same serum, mixed with lime-water, was not clouded. It follows from this, that the blood contains no sulphuric acid, and only a vestige of the phosphoric. In my *Treatise on Animal Chemistry*, I have endeavoured to prove, that the phosphates, as well as the lactates, are always produced by the spontaneous decomposition of animal substances, and that the small quantity of each which is found in the blood is carried thither by the absorbent system, in its progress to the secretions, through which it is discharged from the body, and hence the secretions contain always a much larger proportion of these acids.

Not to be too diffuse, I shall pass over the description of the methods I employed, to ascertain the respective proportions of the contents of the serum, and shall only give the results:—

A thousand parts of serum I find to consist of

Water		905·00
Albumen		79·99
Substances soluble in alcohol, viz.		
Lactate of soda, and extractive matter	6·175	8·74
Muriate of soda and potash	2·565	
Soda and animal matter soluble only in water		1·52
Loss		4·75
		1000·00

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not, it has been considered as a single substance, and has received the name of *osmazome*. One of the component parts of this extract is *lactate of soda*, and the other, with which it is intimately combined, is an animal matter that may be separated by means of tannin. To prove the presence of lactic acid, dissolve the whole in alcohol, and add a mixture of sulphuric acid much diluted with alcohol, as long as there appears any precipitate, which is sulphate of potash or soda. Digest this spirituous solution (which contains muriatic, sulphuric, lactic, and sometimes phosphoric acid) with carbonate of lead, and all the above acids will unite with the oxide of lead, but of these only the lactate will be soluble in alcohol. Decant the alcoholic solution of lactate of lead, separate the lead by a stream of sulphureted hydrogen gas, and by evaporating the clear liquor, the lactic acid will remain in the state of an acid syrup.

I washed the albumen well in this experiment, digested it in muriatic acid, and then burnt it to ashes, which were almost exactly equal in quantity to the ashes produced by the combustion of the same weight of colouring matter. But the ash of the albumen was white, and did not show a particle of iron. I found a trace of soda, but the greatest part was phosphate and carbonate of lime, with a little magnesia. It is clear, therefore, that the earthy salts found in the ashes of coloured blood had not been dissolved in the blood, nor even existed as salts in the blood, from which they were obtained by means of combustion. Hence we may conceive, how the blood can produce and deposit, in the animal economy, the earthy phosphates, which, however, are not soluble either in pure water or in the blood; and hence too we may infer, that the production of bone cannot be considered as a simple crystallization of a salt, conveyed by the blood in a state of solution, but requires us to suppose the decomposition of the animal matter of the blood, as well as in any other secretion.

All the authors who have written on the blood, assert that gelatine is one of its component parts. This, however, is a mistake, and arises from the gelatinous appearance of the albumen, as I have never been able to detect a particle of gelatine in blood; and, as far as my researches extend, I have found gelatine to be a substance altogether unknown to the economy of the living body, and to be produced by the action of boiling water on cartilage, skin, and cellular membrane, substances which are totally distinct from fibrin and albumen.

On Human Blood.

The blood of man perfectly resembles in composition that of the ox, but the coagulum of human blood is more easily decomposed by water, and the fibrin thus obtained is more transparent. When dried, it amounts to no more than 0·75 from 1000 parts. Human fibrin has the same chemical properties with that of the ox, but is more readily incinerated: the white ash consists of the phosphates of lime and magnesia, a little carbonate of lime, and soda.

The colouring matter of human blood is also chemically the same with that of ox blood, but is much more easily reducible by fire to the same yellow ash, which seems to show that it contains less azote or ammonium. A hundred parts of dried colouring matter of human blood gave 15· parts of ash, of which 3 parts dissolved in water, and were alkaline, and when saturated with acetic acid, and mixed with muriate of barytes, it left a copious precipitate of phosphate of barytes, soluble in an excess of muriatic acid. I found in this acetic solution no trace either of muriatic acid or of potash. It appears, therefore, that soda and phosphoric acid, as well as the earthy phosphates, are products of the combustion. As to the portion of the ash of colouring matter, which was insoluble in water, it consisted of the same substances in nature and in proportion, as that of the ash of the colouring matter of ox blood.

The serum of human blood is composed (according to my experiments) of

Water		905·0
Albumen		80·0
Substances soluble in alcohol, viz.		
Muriate of potash and soda	6	10·0
Lactate of soda, united with animal matter	4	
Substances soluble only in water, viz.		
Soda, phosphate of soda, and a little animal matter		4·1
		999·1 *

Human albumen is more easily incinerated than that of the ox, and contains more soda and phosphate of soda. A hundred parts of the dried albumen give twelve parts of calcined ash.

The muriates found in human blood are triple the quantity of those in ox blood, owing doubtless to the salt consumed by man in his food. Human blood also contains a larger proportion of muriate of potash.

On the whole, the great agreement in the composition of human and of ox blood is remarkable, and explains to us the possibility of the phenomena observed in the experiments in transfusion.

General Results of the Analysis of Blood.

1. Blood is composed of one portion which is liquid and

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* I cannot refrain here from comparing my analysis with that made in this country by Dr. Marcet, and given in the second volume of the Medico-Chirurgical Transactions, p. 370.

Dr. Marcet finds the following ingredients:—

Water	900·0
Albumen	86·8
Muriate of potash and soda	6·6
Muco-extractive matter	4·0
Sub-carbonate of soda	1·65
Sulphate of potash	0·35
Earthy phosphates	0·60

A more perfect agreement cannot be expected in the analysis of substances liable to so many accidental differences, particularly in the quantity of water, which in the blood depends so much on the proportion of liquids taken into the stomach. It is clear that Dr. Marcet's *extractive matter* is impure lactate of soda; and I must also observe, that the sulphate of potash and the earthy phosphates found by him in the ashes of serum, are probably, for the reasons above-mentioned, formed by the process of combustion.

homogeneous, and of another which is only suspended and spontaneously separates when at rest.

2. The liquid part is a solution of much albumen and a little fibrin, both combined with soda. It also contains some other saline and animal substances, but in very small quantity.

3. The portion which is suspended is the colouring matter. It differs from the albumen chiefly in its colour, and its insolubility in serum. The colour seems to be owing to iron, of which it contains $\frac{1}{3}$ per cent. of its weight, but which cannot be separated from it as long as it continues to be colouring matter. This separation can only be affected by combustion, or by the concentrated acids, both of which agents entirely decompose the substance with which the metal was combined. The colouring matter cannot be artificially produced by uniting albumen with red subphosphate of iron.

4. Fibrin, albumen, and colouring matter, resemble each other so closely, that they may be considered as modifications of one and the same substance.* I shall in future call them *albuminous contents of the blood*, when speaking of them collectively. These three substances *produce* when decomposed, but *do not contain*, earthy phosphates and carbonate of lime; and, indeed, the entire blood contains in solution no earthy phosphate, except perhaps in too small a quantity to be detected.

5. The albuminous contents of the blood will unite with acids, and produce compounds, that may be termed saline; these, when neutralized, will dissolve in water, but separate on adding an excess of acid. The acetous and phosphoric acids, however, must be excepted, as an excess of either of these forms a compound soluble in water. Nitric acid, digested with the albuminous contents, forms an insoluble compound consisting of the albumen in an altered state, and of the nitric and the malic acids. This property of combining with acids is retained in some instances by the albumen after it has undergone the changes produced in the secretory organs; as for instance in the peculiar matter of the bile, the curd of milk, &c.

6. The blood contains no gelatine.†

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* One of the most striking points of difference exists in the property which the colouring matter has of absorbing oxygen, and thereby experiencing a very remarkable change of colour. Serum absorbs very little oxygen, and only in proportion as it is decomposed. Can the iron in the colouring matter give it this property? This is probable; but we shall never arrive at any accurate knowledge of these phenomena, without first analyzing these elements of the animal kingdom with the most scrupulous exactness. It is then, and not till then, that we may form conjectures; at present they are useless.

† It gives me great pleasure to find that an English chemist, Dr. Bostock, has arrived at the same conclusion, without any previous knowledge of my work, and (owing to a delay in the publication) prior to its appearance. See Dr. Bostock's experiments, published in the first volume of the Medico-Chirurgical Transactions.

insoluble in alcohol contains a distinguishable quantity of phosphate of soda, a little of a similar animal matter to that found in the secretions, and also the earthy phosphates which were held in solution by the lactic acid, and were precipitated by the action of the alcohol. The urine possesses also a number of other substances, which will be specified when describing this excretion in particular.

Having thus given some general views of the composition of all the secreted fluids, I shall proceed to give a short account of each individually.

(*To be continued.*)

Article VII.

Analysis of the Chinese Gong. By Thomas Thomson, M.D. F.R.S.

The Chinese *gong* is an instrument that has been long known in this country, though I am not aware of any account of its constituents having been hitherto published. My friend Dr. Reid Clanny, of Sunderland, having some time ago sent me a specimen of this metal, with a request that I would ascertain its composition, I thought it might gratify my readers if I stated the result of my trials in the *Annals of Philosophy*.

The Chinese *gong* is a large circular instrument, somewhat similar in shape to a tambourine, excepting that it is entirely of metal, and that the face is not flat, like the face of a tambourine, but somewhat convex. The metal of which it is composed has exactly the appearance of bronze. It varies in thickness in different parts, from the $\frac{1}{15}$th to the $\frac{1}{20}$th of an inch in thickness. The surface is irregular, and bears evident marks of the hammer; yet the metal is brittle, and very elastic. When broken it has a granular texture, and its colour is rather whiter than any part of the surface exposed by means of a file.

This brittleness of the gong, although it had obviously been made under the hammer, naturally suggested the idea that it would be found malleable at some temperature between that of the atmosphere and a red heat; and I was going to undertake a course of trials in order to determine the point: but Dr. Wollaston informed me that he had already made the experiment, and found the gong quite malleable at a temperature considerably below that of a red heat. He had been induced to undertake his experiments in consequence of a gong belonging to Sir Joseph Banks having cracked. Dr. Wollaston determined the composition of the metal, made a quantity of similar alloy, mended the crack, and restored the tone of the instrument. The crack, however, afterwards extended, as always happens in brittle and very elastic bodies.

Every body, I presume, knows that the gong is used as a kind of substitute for a bell; that the tone is at first low; but that, by skilful beating, it becomes higher and higher, till it makes the whole house shake with the loudness of its tones.

One of the most remarkable circumstances belonging to the gong is its specific gravity. I found it 8·953. Upon taking the specific gravity of a piece of British bell-metal I found it 8·368. This, however, was a much more complicated alloy than the Chinese gong. I found it composed of copper, tin, lead, and zinc. The proportion of copper was nearly the same as in the gong; but the other constituent, which in the gong is nothing but tin, I found in the British bell-metal composed as follows:—

Tin	10·1
Zinc	5·6
Lead	4·3
	20·0

The gong is composed (very nearly) of 80 parts copper and 20 of tin. The specific gravity of the purest copper I have ever seen was 8·895: but Cronstedt informs us that Japan copper is of the specific gravity 9·000. Now as it is not unlikely that this may have been the copper used in China for manufacturing the gong, we shall suppose the specific gravity of copper to be 9·000. The specific gravity of pure tin is 7·299. Now let the weight of the copper in the alloy be a, and its specific gravity α; let the weight of the tin be b, and its specific gravity β. To find the specific gravity of the alloy, supposing no change of density, we have this formula: sp. gr. $= \frac{\alpha \beta (a + b)}{\beta a + \alpha b}$. But in the present case $a = 4$, $\alpha = 9·000$, $b = 1$, and $\beta = 2·799$. Hence the specific gravity of the alloy ought to be 8·337: but it is, in fact, 8·953; very nearly equal to that of Japan copper, and higher than that of any European copper I ever met with: so that a very considerable condensation has taken place, a condensation amounting to more than $\frac{1}{14}$th of the whole. This curious fact of the great increase of density was known before, from the experiments of Mr. Briche; and, according to him, the specific gravity is a maximum when the alloy is composed of 100 copper and 16 tin. When the metals are united in that proportion, the density has been found no less than $\frac{1}{10}$th greater than the mean. The Chinese gong contains a much greater proportion of tin than 16 to 100 of copper. Hence in it the increase of density is not so great as $\frac{1}{10}$th.

The analysis of bell-metal is so simple, and so well known, that it may seem superfluous to enter into details; but as such details are the only means of detecting the mistakes into which the experimentor may fall, I think they ought never to be neglected.

101·85 grains of the gong were put into a glass phial, and two ounces of moderately strong nitric acid poured over it. The acid speedily became green, an effervescence took place, and a white powder appeared. When the action of the acid appeared at an end, it was decanted off, and two ounces more of the same acid substituted in its place. The whole copper was dissolved by this last portion, and a white powder remained at the bottom of the phial. The acid solution was decanted off, and the white powder carefully washed with distilled water.

The acid solution was evaporated to a small quantity, to get rid of the excess of acid. During the evaporation a portion of white powder precipitated. It was separated by the filter, and mixed with the first portion. The white powder thus obtained was dried in the open air. It acquired, when dry, a tinge of green. Conceiving that this might be owing to the presence of copper, I digested it for a week in nitric acid; but no copper was taken up. It was then dried, and exposed to a red heat. In that state it was yellow, and weighed 25·36 grains. Now this yellow powder was peroxide of tin, which is a compound of 1 atom tin and 4 atoms oxygen, or of 14·705 tin + 4 oxygen: and $14{\cdot}705 : 4 :: 25{\cdot}36 - x : x = 5{\cdot}423$. Hence the quantity of tin in the oxide was 19·937; but the quantity of gong analysed was 101·85 grains: 100 grains, therefore, contain 19·573 grains of tin. This approaching so near 20, I have supposed that $\frac{1}{5}$th was the proportion of tin intended to be mixed with the copper.

The nitric acid solution was evaporated to dryness, redissolved in water, and tried for silver by nitrate of silver, and for lead by sulphate of soda. No precipitate appeared in either case. Hence I consider myself entitled to regard the gong as free from both silver and lead. To see whether any zinc was present, the nitrate was mixed with as much sulphuric acid as was sufficient to saturate the copper. It was then evaporated to dryness, redissolved in water, and set aside for spontaneous crystallization. Regular crystals of sulphate of copper were obtained to the very last drop. Hence no zinc was present. The gong, by my trials, was an alloy of

Copper	80·427
Tin	19·573
	100·000

ARTICLE VIII.

Memoir on the Determination of the Specific Heat of the different Gases. By MM. F. Delaroche, M.D. and J. E. Berard.

(With a Plate.)

(Continued from p. 138.)

SECTION FIRST.

Explanation of the Method followed in our Researches.

§ I.—Description of the Calorimeter.

The object which we had in view in the experiments that we are about to describe was the determination of the specific heat of several gases, according to the meaning usually attached to that word; that is to say, to determine how much caloric is necessary to raise them from a given temperature to a higher temperature, likewise given; or, which comes to the same thing, how much heat they give out in passing from the higher temperature to the lower. We have not attempted to determine the effect produced by the change of their specific heat, in consequence of a change of their temperature; a change which ought to be more sensible in the gases, in consequence of their great dilatability, than in other bodies, and which Gay-Lussac has proved to exist, though it would have been very difficult for us to determine it by means of our apparatus.

The solution of the question, such as we consider it, is not quite so simple as may at first sight appear; but presents considerable difficulties, what process soever we pursue. We hesitated for some time about the plan that we ought to follow in order to gain our object. We rejected at once the process of Crawford, and likewise the calorimeter of ice: 1st. Because it did not seem susceptible of precision when such small quantities of heat are employed as those that the gases give out in cooling some degrees. 2d. Because, when we employ it, there is almost an impossibility of determining the temperature of the gases when they enter the interior chamber. 3d. Because it is necessary to use dry gases, which would make the apparatus too complicated. We conceived the idea of employing a calorimeter, in which, instead of determining the heat disengaged during the cooling of the gases by the ice melted, we should determine it by the quantity of water or ether evaporated; but we found this process attended with such difficulties as induced us to renounce it. At last we determined to employ another, founded on the following considerations.

Suppose we have a constant and uniform source of heat, the whole of which acts upon a body, A, suspended in the air; this body will become gradually hotter and hotter, till it reaches a temperature when it loses as much heat as it receives. At this point its temperature will become stationary, if that of the air does not vary.

On the other side, it is a principle generally admitted, and the justice of which cannot be questioned when confined to small differences of temperature, that the quantity of heat lost every instant by a hot body suspended in the air is proportional to the excess of its temperature above that of the surrounding air.

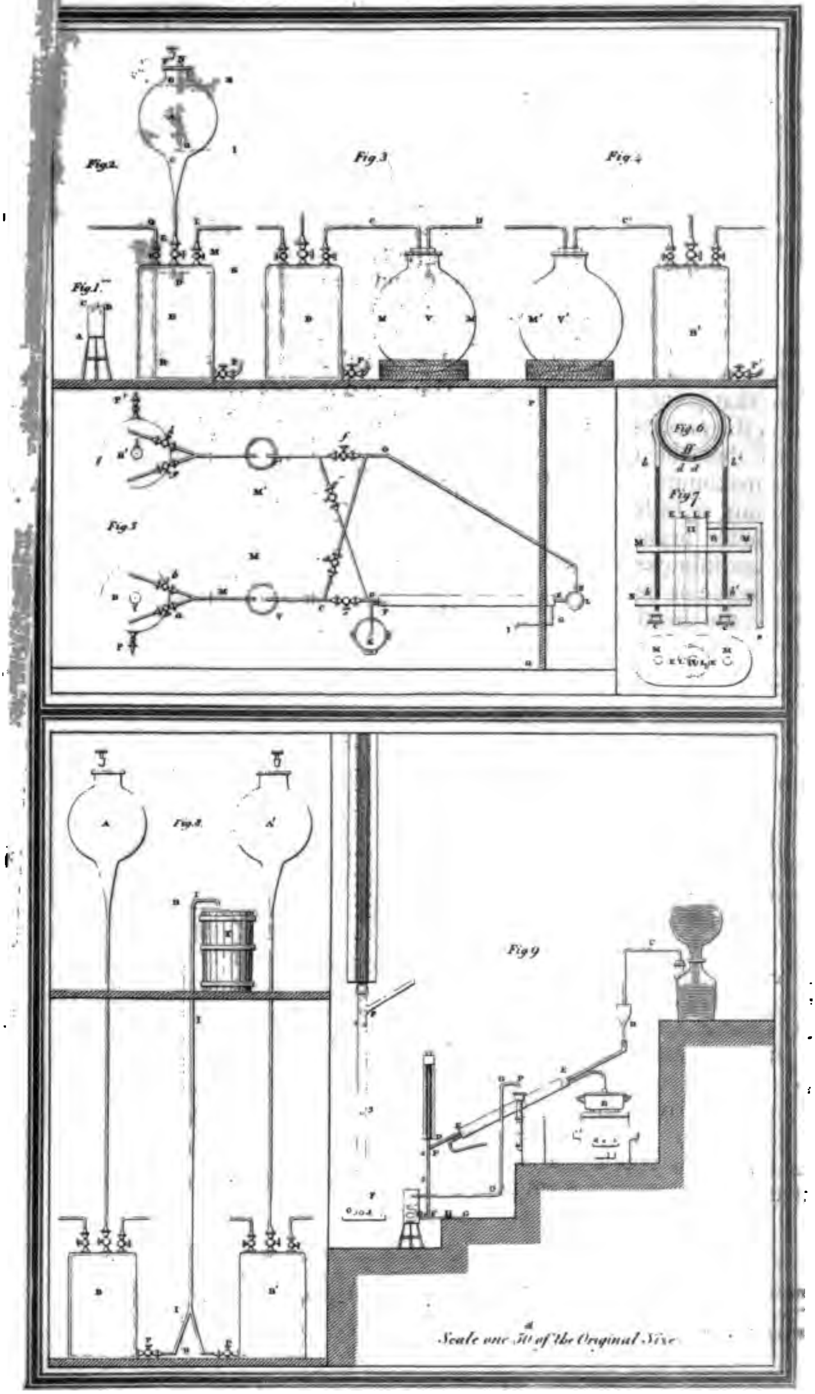
It is evident, from these two principles, that if we subject a body, A, to the action of different uniform sources of heat, the ratio of their intensity will be equal to that of the excess of the temperature of the body, A, when it becomes stationary, above that of the ambient air; since the body, A, arrived at this maximum, receives at each instant the heat which it loses.

Now let us conceive a thin copper cylinder, A B, (Fig. 1, Plate X.) 6 inches long, and 3 in diameter, filled with distilled water, and traversed by a serpentine of about 5 feet in length, forming 8 spiral turnings, the two ends of which open without the vessel, the one at the top, the other at the bottom. If we make a regular current of gas traverse this serpentine, maintained before its entrance at an elevated and constant temperature, this current may be considered as an uniform source of heat, and the cylinder, A B, as the body, A. Of course, if we repeat the same experiment upon each of the gases, each current will raise the temperature of the cylinder, A B, to a fixed point, where it will remain stationary; and it follows from the principles announced above, that, reckoning from this point, the excess of the temperature of the cylinder, A B, above that of the ambient air, will be proportional to the quantity of heat given out by the current of gas that passed through the cylinder. Hence we shall obtain by this method, with great exactness, the relative specific heats of the gases subjected to this kind of experiment. There are, likewise, two methods of comparing them with water.

The first consists in subjecting the cylinder, A B, which we shall afterwards call the *calorimeter*, to the action of a current of water, perfectly regular, and so slow that it will hardly produce a greater effect than the current of the different gases.

The second method consists in determining by calculation the real quantity of heat which the calorimeter, come to its stationary temperature, can lose in a given time; for since after it reaches this point it does not become hotter, though the source of heat continues to be applied to it, it is evident that it loses as much heat as it receives. We shall employ, in the sequel, these two methods in succession.

It is obvious that it would have been exceedingly tedious to



Apparatus for measuring the Specific heat of GASES.

raise the temperature of the calorimeter as high as it could be raised by the sole effect of a current of gas, and that the observation of the rate at which it rose during such a process would have been of no utility. We thought it better to raise it artificially, by means of a lamp of spirit of wine placed under it, to a point which, from preliminary trials, we knew to be near that when the temperature would become stationary. We then made the current of hot gas pass through it, and observed its temperature every ten minutes. Notwithstanding this precaution, it would have been still very tedious to have waited till the cylinder had reached its true maximum, and even difficult to have determined that point by inspection. We found it more convenient to stop the process when, from the slowness of the heating, we judged that the calorimeter was within 0·5, or 0·7 of a degree of its maximum. Then raising the calorimeter till it passed the maximum a little, and making the current of gas to pass, the calorimeter began to cool. We observed the rate of cooling every ten minutes, and stopped the process when we judged that it had come as near the maximum, on the one hand, as we had got by the heating process, on the other. Taking the mean between these two points, we obtained exactly the maximum point, at which the calorimeter would have remained stationary if the current of hot gas had been long enough continued.

We determined the temperature of the calorimeter by means of a thermometer with a cylindrical bulb, almost equal in length to the height of the calorimeter. An opening, C, made in the cover allowed the stem of the thermometer to pass out. The thermometer had a very fine bore. Every degree* was 0·39 inch in length, which was divided into 10 parts, each of which, by a little practice, we were able to subdivide by the eye into other 10 parts. We were certain of not committing an error equivalent to 2 of these last, or to 0·02 of a degree. As it was of equal importance to know with accuracy the temperature of the air which surrounded the calorimeter, we suspended, at the distance of 3 inches from it, a very sensible thermometer.

Before entering into the details of our experiment, we must describe the methods which we employed in order to obtain an uniform current of gas, to give it a constant temperature, to determine its temperature when it entered and left the calorimeter, and to appreciate the causes which might, independent of that current, raise the temperature of the calorimeter.

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* The degrees alluded to in the paper are those of the centigrade thermometer, each degree of which is to a degree of Fahrenheit, as 9 to 5; so that had the thermometer been divided into Fahrenheit's degrees, each degree would have been 0·216 inch in length.—T.

§ II.—*Apparatus employed to make a regular Current of hot Gas pass through the Calorimeter.*

We employed, in order to procure an uniform current of gas, a gazometer, which we believe was invented by Dr. Wollaston, and which unites a great deal of simplicity with the greatest precision. A few words will explain the nature of this ingenious instrument. Suppose a globular glass vessel, A, (Fig. 2,) filled with water, and placed above a reservoir of glass or metal, B, filled with any gas insoluble in water. Let these two vessels communicate by a vertical tube, C D, which may be shut by a stop-cock, E. Suppose, likewise, that the upper surface of the water contained in A is G H. It is evident, that if we open the stop-cock, E, the water will fall into the vessel, B, and drive out the gas, which will make its escape by the mouth, L, (the stop-cock, M, being open). It is equally obvious that the force with which the water of the vessel, A, will run down, at first equal to the column of water, H K, will diminish in proportion as the surface of the water, G H, sinks. But then, if we shut exactly the mouth, F, and establish the communication of the vessel, A, with the external air by the tube, N O, open at its two ends, the air, in order to introduce itself into the vessel, A, and fill up the place of the water that has run out, must overcome the pressure of the column of water, H I. Of course the water will only run into the vessel, B, with a force equal to the column H K, minus H I, that is to say, the column I K, which is a constant quantity, as long as the surface of the water continues higher than O I. Now suppose the reservoir B be totally emptied of gas, and filled with water from the vessel, A. Shut the stop-cocks, E, M, and introduce by the tube, Q R, which goes to the bottom of the vessel, a constant current of gas, coming from a similar gazometer. In these circumstances, if we open the stop-cock, P, to let out the water from the reservoir, B, it is clear that this gas, in order to get into the reservoir, B, is obliged to overcome a resistance represented by the column of water which this reservoir contains. On the other hand, it is attracted by an equal force, namely, the force with which this water tends to run out by the stop-cock, P, and which is represented by the same column. These two forces being equal, and opposite, it follows that the regularity of the gas entering by the tube, Q R, will not be disturbed, and that the reservoir, B, will be filled with gas coming from the other gazometer, without having any effort to overcome. The stop-cock, E, remaining shut during the whole of this operation, there will be time to open the mouth, F, and to fill the vessel, A, with water, in order to recommence the operation. It is easy to see that with two similar gazometers we may make a certain quantity of gas pass from the one to the other as often as we please, without interruption.

When the gas which fills the reservoir, B, is not atmospherical air, when it is hydrogen gas, for example, if we pass it often from one gazometer to another, it will disengage the air contained in the water, and take its place so that the purity of the gas will be injured. It would have been impossible to have produced a current of carbonic acid gas, nitrous oxide gas, or olefiant gas, in this manner, though they are but imperfectly soluble in water. This difficulty would have obliged us to renounce this kind of gazometer, if we had not found a method of obviating it. This method consists in putting only atmospherical air into the reservoir, B, and in introducing the gas which we wish to circulate into a bladder, V, (Fig. 3,) inclosed in a globular vessel, M, communicating by the tube, C, with the reservoir, B. If in this state of things we suppose a regular current of atmospherical air proceeding from the reservoir, B, to the vessel, M, through the tube, C, as the vessel, M, is accurately shut, the air will press uniformly on the bladder, and there will issue out through the tube, D, a regular current of the gas contained in the bladder.

If, on the other hand, we suppose that the constant current issuing from the bladder, V, enters into another bladder, V′, (fig. 4.) which is empty, and placed, in the same manner as the first, in another globular vessel, M′, which is full of air, and communicates with the reservoir, B′, of the other gazometer, by the tube, C′, which reaches down to its bottom. The reservoir, B′, being full of water, and its stop-cock, P′, open, the bladder, V′, will become gradually filled with gas, and will drive the air of the globular vessel, M′, into the reservoir, B′, in an uniform current. It is now easy to form an idea of the apparatus which we employed, and of which we have given a vertical projection in fig. 5.

B and B′ are the two lower reservoirs of the two gazometers. The reservoir, B, is supposed full of air, and B′ full of water. V is a bladder filled with gas, whose specific heat is to be determined, hydrogen gas, for example. The corresponding bladder, V′, is empty; *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, are the stop-cocks. Let us suppose *a*, *c*, *f*, *h*, alone open, if we make the gazometer, B, act, a regular current of common air will flow out of B, and pass into the globular vessel, M. It will compress the bladder, V, and force out a current of hydrogen gas. This gas will pass through the tube C D E, about 40 inches in length, and which is surrounded by a larger tube, F G. This last tube is kept filled with the steam of boiling water by means of a small boiler, K, filled with water, and kept constantly boiling. The steam passes through the tube, K F, into the tube, F G, passes through the whole of it, and makes its escape by the tube, G I. The part, D E, of the tube through which the gas passes, is long enough to enable the gas during its passage to acquire very nearly the temperature of boiling water. The gas, when it leaves this tube, passes into the calorimeter, L, where it gives out its heat, and then issues out by the tube, N O, which conducts it to the bladder, V′. It fills this bladder, driving the air of the vessel, M′, into the reservoir, B′, by a tube which plunges to the bottom of the reservoir.

When all the air has been driven out of the reservoir, B, and water has been substituted in its place, then the bladder, V, is empty, and the vessel, M, full of air from the reservoir B. The bladder, V′, is full of hydrogen, and the reservoir, B′, full of air, from the vessel, M′. If we now shut at once the stop-cocks *a*, *c*, *f*, *h*, and open *g*, *e*, *d*, *b*, and put the gazometer, B′, into action; then the air issuing from the gazometer by the stop-cock *g*, will fill the vessel, M′, press upon the bladder, V′, and cause an uniform current of hydrogen gas to pass through the stop-cock *e*, into the tube, D E, where it will be heated. It will then pass through the calorimeter, and issuing out by the tube, N O, it will pass through the stop-cock, *d*, into the bladder, V, will fill it, and drive the air of the vessel, M, into the reservoir, B. Thus matters will be brought to their primitive state, and we may commence the process as at first. With a bladder filled with hydrogen gas we may pass an uniform current as long as we please through the calorimeter; and we know, from experience, that, notwithstanding the continual agitation given to this gas by making it move so frequently through so long a circuit, it does not contain, after having circulated for six hours, three per cent. of impurities.*

The greatest part of the apparatus which has been described is contained in the same room; but the calorimeter, the ends of the tubes D E, F G, and a part of the tubes G I, N O, are in another room, separated from the first by the door, P Q, which has proper holes in it to allow the tubes to pass. This second room being but seldom opened, the air which it contains, and which surrounds the calorimeter, is seldom agitated, and its temperature scarcely varies.

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* The gas continually agitated in moist bladders was always saturated with humidity at the temperature of the bladders. Though we took no precautions to dry it, we do not think that the vapour produced any sensible effects upon our results: 1st. Because none could be deposited in the calorimeter, which was always a good deal hotter than the bladders. 2d. Because it could only act as a mixed gas; and although the vapour of water has twice as great a specific heat as the same volume of air, we made our experiments at a temperature too low (48° Fahrenheit) for it to produce any sensible effect.

§ III.—*Method of determining the Heat given out by the Gas in passing through the Calorimeter.*

In the description which we have given of our gazometers we have explained the method employed to heat the gases. It is obvious, that whether they reached the temperature of boiling water, or remained a little colder, they would always acquire a constant temperature. It was necessary to know this temperature before they entered into the calorimeter. It must have been less than that of boiling water; for it was not certain that it would acquire that temperature in passing through the tube; and even if it had, as it had to pass through a small portion of the tube not surrounded with steam, before getting into the calorimeter, its temperature would necessarily sink somewhat in that place. It seems, at first sight, that it would have been easy to determine this temperature at the entrance of the gas into the calorimeter, by means of a thermometer placed in that spot, in the middle of the current; but we convinced ourselves, by experiments too tedious to describe here, that a thermometer placed in such a situation always stands lower than the true temperature of the current. It will be easy to perceive that this must be the case, if we reflect that bodies placed in an aeriform fluid, being influenced as to their temperature by the surrounding bodies, assume a middle temperature between that of these bodies and the aeriform fluid with which they are surrounded. In the present case, the tube, in the centre of which the thermometer is placed, being colder than the gas, must act upon the thermometer by means of its radiating quality, and sink its temperature. We ascertained, by experiment, that a thermometer with a gilt bulb stood always higher in such a situation than a common thermometer.

This difficulty of appreciating the temperature of a current of gas at its entrance into the calorimeter induced us to make the tube between the vapour tube and the calorimeter as short as possible. By this means we succeeded in having the temperature of the gas very little lower than that of boiling water; and as we were certain that this temperature could neither be lower than that indicated by the thermometer, nor higher than that of boiling water, it is obvious that by taking the mean between these two temperatures we could not commit an error of great importance.*

We did not experience the same difficulty in determining the temperature of the gases after they came out of the calorimeter. A thermometer placed at the end of the serpentine showed us that it gave out the whole excess of its heat, and that it issued out exactly at the temperature of the water of the calorimeter.

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* The thermometer usually stood between 199° and 204° Fahrenheit.

Hence we concluded that the heat given out by the gases was equal to the excess of their temperature at their entry into the calorimeter, such as we determined it, above that of the calorimeter.

§ IV.—*Influence of the Tube which served to heat the Gases upon the Temperature of the Calorimeter.*

An inconvenience attended the shortening of the intermediate tube between the vapour tube and the calorimeter. The calorimeter was heated directly by communication, independent of the heat which it received from the gas which circulated in its interior. Now, as notwithstanding all the precautions that could be taken to measure the heat received by the calorimeter from that cause, some error might have been committed in the measurement, it was necessary to make it as little as possible, that it might be neglected without risk. On this account we employed for that part of the tube a tube of glass, because that substance is a bad conductor of heat. This tube, 0·8 inch in length, and 0·3 inch in diameter, was fixed to the apparatus in the following manner, a little complicated in appearance, but very easily executed, and having the great advantage of rendering lutes of no use.

The sides of our calorimeter are strengthened below by a pretty thick plate of copper, about 0·8 inch high, firmly soldered on, and covering nearly one-half of the circumference, as may be seen in fig. 6, which represents a horizontal section of the calorimeter through its lower part. This plate has fixed to its two extremities two rods, *b b*, *b′ b′*, six inches long, and cut half their length into the threads of a screw, upon which are fixed the nuts, C, C′. The middle of this plate is pierced with a hole, *d d*, which coincides with the orifice by which the inferior part of the serpentine opens outwards. Round the hole is a flat place, *f f*, destined to receive a piece of leather.

Fig. 7 represents the extremity of the tube filled with vapour, which serves to heat the gases. This extremity is of copper. The plate, E E, which terminates it, is pretty thick, and pierced in its middle with an opening, I I, communicating only with the interior tube, which contains the gas. This opening is surrounded externally with a flat place, L L, which receives a round piece of leather. Immediately in the neighbourhood of the terminal plate is the tube R S, which opening into the inside of the vapour tube, allows the vapour and the condensed water to escape, and carries them to a distance from the calorimeter. It is the same tube that is represented by G I in the horizontal section of the apparatus, fig. 5.

M M, N N, are two oval plates, pierced in their centre by a circular opening, which receives the vapour tube, to which they are firmly fixed. At each of their extremities is a hole, sufficiently large to give passage to the rods, *b b*, *b′ b′*, fixed to the calorimeter, and represented in fig. 6.

It is easy to see that when the extremity of the vapour tube is presented to the calorimeter, and the metallic rods are made to pass through corresponding openings in the wooden disks, M M, N N, the opening, I I, of the gas tube will be exactly opposite to the opening, *d d*, of the serpentine. If we interpose between the two openings a tube of glass of the requisite size, and such that its extremities, ground with care, press against the disks of leather which cover the flat faces, *f f*, L L; and if, by means of the nuts, C, C′, we force the extremities of the vapour tube to approach as near as possible to the calorimeter, then the tube of glass firmly fixed between the two disks of leather will establish a communication between the gas tube and the calorimeter, and will prevent all gas from escaping in any other direction.

In describing our apparatus we have said that the calorimeter was in a different room from that of the rest of the apparatus, and that the communication took place by means of a hole made in the door (fig. 5). As a part of the vapour tube was in the same room with the calorimeter, it was screened from the effect of radiation by covering the hot tube with a box of tin plate, properly disposed to allow no radiant heat to pass, and that the air which surrounded this tube should have no communication with that which surrounded the calorimeter.

Notwithstanding all these precautions we could not prevent the calorimeter from being a little heated by the vapour tube by immediate communication. We endeavoured to determine to how much that cause of heat amounted. We have ascertained that when it acted alone it amounted to 3·1° (5½° Fahrenheit); and we think we may, without sensible error, reckon it at 2·5° (4½ Fahrenheit) when the calorimeter was heated likewise by the current of hot gas.*

The preceding details, perhaps, will be thought rather long; but we considered it improper to omit them, because they will enable the reader to appreciate the accuracy of our results. We hope that the experiments which we shall next describe will merit the confidence of philosophers and chemists.

(*To be continued.*)

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* See note 1 at the end of this paper.

Article IX.

Analysis of the Chyle of the Horse. By M. Vauquelin.*

M. Verrier, Clinical Professor at the Veterinary School of Alfort, had the goodness to procure me, at my request, the chyle of two horses, which he destroyed by blowing air into the left jugular. These horses, though exhibiting some symptoms of the glanders, were, notwithstanding, in good condition, and had the appearance of good health. One of them, aged four years, was a stallion; the other, aged eight years, was a gelding. Both of them had eaten abundantly of hay and oats before their death. They were opened immediately after death, and the thoracic duct was tied near its insertion into the right axillary.

The first of these horses furnished, by means of a puncture towards the middle of the thoracic duct, a considerable quantity of chyle, of a red colour, but less intense than that of blood. There was obtained, likewise, by puncturing one of the sublumbar branches, a considerable quantity of chyle as white as milk.

The second horse, by puncturing the middle of the thoracic duct, yielded a quantity of reddish chyle; but it was not practicable to puncture the sublumbar branches, and procure white chyle, as was done with the first horse.

As I examined each of these portions of chyle separately, I shall, in order to be better understood, denote each by numbers, in the order according to which I propose to treat of them. I shall call No. 1 the portion of chyle drawn from the middle of the thoracic duct of the first horse; No. 2, the white portion of chyle furnished by the sublumbar branches of the same animal; and No. 3, the chyle obtained from the thoracic duct of the second horse.

When we examine what anatomists and physiologists have said of chyle, we find very little that can throw light upon its chemical nature. They have all described, with more or less exactness, its physical characters, and the modifications which it undergoes in different circumstances. I shall here give a short abstract of their remarks upon this subject.

Lister observed chyle swimming, like a species of oil, on the surface of blood and of serum. Wepfer observed that a kind of cream formed on the surface of chyle. Bourdon, Pecquet, Bartholin, Monro, &c. announced the coagulation of chyle, both in the vessels which contain it, and in the open air. Bohn, Berger, Ash, described butyraceous globules swimming in an aqueous liquid. They conceived that chyle contains a cheesy matter, which they considered as more earthy than the other constituents, and by the precipitation of which they accounted for the calculous concretions found in the reservoir of the chyle by Schars, in the thoracic duct by Ledran, and in the vessels by Goëlik. Mart, Musgrave, and Lister, announced that indigo, mixed with the food, and given to animals, tinged the chyle of a blue colour; and this observation was confirmed by Baller, Gould, and Fœlix. Mattei says that he communicated a red colour to chyle by giving beet to animals for food. Several observers speak of a green colour in the chyle of herbivorous animals. M. Hallé, to whom we are indebted for several interesting experiments on the same subject, never observed in the chyle of dogs to which he had given pastry coloured blue, red, and black, by means of vegetable substances, any trace of these colours. The physical properties which we have observed in the chyle of the horse are very similar to those which M. Hallé observed in the chyle of dogs. Such are the principal facts which are to be found in authors respecting chyle. I now pass to the analysis.

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* Translated from the Annales de Chimie, vol. lxxxi. p. 113.

CHYLE, No. 1.—*Physical State.*

This portion of chyle, of a red colour, was coagulated when brought to me. It contained a small quantity of liquid, less coloured than the coagulated portion, which was itself of a much lighter colour than blood. It was red, and semitransparent, like currant jelly badly boiled; yet its consistence was pretty firm.

Chemical Examination of the Liquid Portion, or the Serum of the Chyle No. 1.

1. This liquid quickly restored the blue colour to litmus, reddened by acids; which proves that it contained an uncombined alkali.

2. Heat and acids coagulated it into a greyish white mass.

3. Alcohol likewise occasions an abundant coagulation in it. The coagulum is white, but acquires a reddish tint by desiccation. It becomes at the same time transparent, and exhibits a vitreous fracture. When thus dried it decrepitates, and melts on red-hot coals, exhaling an empyreumatic ammoniacal odour.

This coagulum dissolves in caustic potash; but the liquor remains milky, and does not become transparent, as happens to a solution of the albumen of blood in the same circumstances. The alkaline solution is precipitated by acids, and at the same time there exhales from the mixture an odour of sulphur, which has some analogy to the smell perceptible in stables.

Chyle, No. 2.—*Physical State.*

This portion of chyle was white and opake, like milk. It contained a coagulum equally white and opake.

After having separated the liquid portion from the coagulum, I washed the latter, and set it aside. The reader will find an account of its properties hereafter.

The Liquid Portion.

This portion, drawn from the sublumbar branches, presented the same properties as the liquid from the red portion of chyle, excepting as to colour. It was coagulated by heat, by acids, by alcohol; the precipitates were redissolved by the alkalies, and the solution remained milky, like the solution of the precipitate from the coloured portion of the same chyle.

The portion of white chyle treated with boiling alcohol was entirely coagulated, as I have mentioned above; but the alcohol retains in solution a small quantity of matter, a part of which it deposites, on cooling, in the form of flocks; but a portion remains in solution, as is shown by the addition of water, which renders the alcohol milky.

Though I have been able to procure only a quantity of this matter insufficient to ascertain its nature exactly, yet I think myself entitled to conclude that it is a species of fat, the insolubility of which in the alkalies shows it to be analogous to what I found in the matter of the brain.

It is, without doubt, this fatty matter which, by its presence, prevents the albumen, in separating from the coagulum, from becoming transparent, like that of blood. It likewise occasions the albumen precipitated by alcohol to remain opake after desiccation. It dissolves in boiling alcohol, and is deposited as the alcohol cools, and it gives the alcohol the property of becoming milky when mixed with water.

As to the matter coagulable by heat, acids, alcohol, &c. there can be no doubt, I think, that it is *albumen.* It forms the greatest part of the chyle.

Thus the liquid parts of the two portions of chyle, Nos. 1 and 2, are of the same nature, if we except the red colour, which does not exist in the chyle of the sublumbar branches, though that chyle contains the materials proper to produce it.

Examination of the Coagulum formed spontaneously in the coloured Portion of Chyle, No. 1.

To obtain the substance which had produced the spontaneous coagulation of that portion of chyle, I washed the mass with water, in the same manner as is done with the coagulum of blood when we want to obtain the fibrin.

I obtained a white substance in flakes and ribbons, which had little consistence, were easily torn, and did not exhibit any fibres in their fracture.

This matter having presented the same properties as those of the coagulum of the white portion of chyle No. 2, what I have said of the one will apply to the other.

Of the Coagulum formed spontaneously in the White Portion of Chyle, No. 2.

This coagulum was white and opake, like boiled white of egg. When washed it stretched under the fingers, and assumed the form of a membrane, possessing a slight degree of elasticity, and its texture presented an appearance slightly fibrous.

This matter digested in a hot solution of caustic potash communicated at first a milky aspect to the liquid; but a greasy matter was gradually deposited, and the liquid became clear. This solution had a smell similar to that of the fibrin of blood dissolved in potash. When this solution is decomposed by acids, it exhales a sulphureous odour, which is developed by albumen in similar circumstances.

The same matter subjected to the action of acetic acid, assisted by heat, produced a liquor white like an emulsion. The matter which rendered the liquid opake was gradually deposited. It appeared to me to be of the same nature as the fatty matter which I noticed in the liquid part of the chyle.

The matter of the coagulum, when dried, and exposed to heat on burning coals, curls up, and exhales ammoniaco-oily fumes. At last it melts, and leaves a bulky coal behind.

From the properties which have been enumerated, it is obvious that the coagulum of chyle has a striking analogy with the fibrin of blood: but I must acknowledge that the resemblance is not perfect; there are some differences, both in the physical and chemical characters of these two substances. The coagulum of chyle has not that fibrous texture, nor the strength and elasticity which belong to the fibrin of blood. It is dissolved more readily and completely by caustic potash, and does not, like fibrin, leave a portion insoluble in that alkali. It seems to be albumen which has begun to assume the characters of fibrin, and has been arrested in its passage: for it unites several properties common to the two substances. If this account be correct, we may conclude that the food taken by animals is converted in the first place into albumen, and afterwards into fibrin. The increasing proportion of fibrin in the chyle as it approaches the place where it mixes with the blood is favourable to this hypothesis. Be this as it may, the rapid change which vegetables experience in the organs of digestion of animals, and their total conversion into animal substances, are truly astonishing. Some hours are sufficient to make the characters and nature of vegetable substances disappear entirely. The powers which preside over digestion must be very active and very powerful.

From this first analysis, it follows, that the chyle of the horse is composed:

1. Of albumen, which constitutes the greatest part of it.
2. Of fibrin, or at least of a substance which resembles it in several respects.
3. Of a fatty substance, which gives to chyle the appearance of milk.
4. Of different salts, such as potash, muriate of potash, and white phosphate of iron; that is, phosphate at a minimum of oxidation.

I ought not to neglect to mention here that M. Emmert was occupied before me with analysing chyle. An extract of his work, by M. Friedlander, is printed in the *Annales de Chimie* for October, 1811; but as there is some difference between the results which we obtained, I thought it right to publish my experiments.

After having written thus far, I received from M. Verrier a new quantity of chyle, the analysis of which furnished nearly the same results. However, as this last portion was not coloured like the preceding, it presented some phenomena which were not perceived before, and which I shall relate in a few words.

Chyle from the Sublumbar Branches.

It was white, like milk. The bulky coagulum which it contained was red in some parts. Water deprived the coagulum of the greatest portion of this colour. Then it resembled fibrin, but had much less tenacity.

The white liquid which surrounded this coagulum being left at rest for 18 hours was found partly covered with drops of a yellowish oil. The same liquid treated with a considerable portion of boiling alcohol let fall a great quantity of albumen. The alcohol assumed a yellowish green colour, and deposited, on cooling, a white oily concrete matter; and yielded, when evaporated, an additional portion of yellowish oil. These phenomena confirm me more and more in the opinion stated above, that the oil of chyle is of the same nature as that which exists in the brain.

The chyle deprived of the fatty matter by repeated digestions in boiling alcohol becomes transparent by drying, as happens to the brain.

A portion of this chyle coagulated by heat, and then incinerated, furnished carbonate of potash, muriate and sulphate of potash, iron, and phosphate of lime.

Among the last portions of chyle sent me by M. Verrier there was one from a horse afflicted with the glanders, which, though taken from the thoracic duct, was colourless, and contained only a small quantity of fibrous matter. This was, no doubt, the reason why it did not coagulate spontaneously, and why it only showed very small traces of that substance by analysis. It was also much less charged with albumen, as was obvious from its greater fluidity, and from its not coagulating when heated. It contained, however, a notable proportion of fatty matter.

This difference seems to announce an exception to the general principle which I pointed out above; namely, that the proportion of fibrous matter increases as the chyle approaches more and more to the place where it mixes with the blood; but this difference may depend upon the nature of the aliments, or perhaps upon the diseased state of the digestive organs of the animal.

I shall finish this paper by remarking, that if the bile, the gastric juice, &c. contribute to the formation of chyle, as physiologists think, these substances must undergo a complete decomposition, since not the slightest traces of them can be perceived in the animal fluid which constitutes the subject of this paper.

I shall observe, likewise, that the comparison which some persons have established between chyle and milk, in consequence of the external appearance of the two fluids, has no real foundation; for chyle contains nothing which agrees exactly with the constituents of milk.

ARTICLE X.

Astronomical and Magnetical Observations at Hackney Wick.

By Col. Beaufoy.

Astronomical Observations, August 11, 1813.

Beginning of the Lunar Eclipse	13	41′	41″	Apparent Time.
Immersion of 1 ψ Aquarius	11	09	40	Mean Time.
Emersion of 1 ψ Aquarius	11	53	01	Mean Time.

These observations were made under very favourable circumstances; and as no diminution of the star's light took place, the opinion of some of the French astronomers that the moon is surrounded with an atmosphere, seems extremely improbable; or at least, admitting it is so, the air must be exceedingly attenuate. The thinness of the moon's atmosphere (if there be any) is further corroborated by the great sharpness of the lunar horns in solar eclipses.

Magnetical Observations.

Latitude 51° 32′ 40″ North. Longitude West in Time 6″$\frac{82}{100}$.

1813.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
July 19	8h	45′	24°	11′	37″	1h	48′	24°	24′	32″	7h	22′	24°	17′	51″
Ditto 20	8	38	24	15	20	1	55	24	21	40	7	10	24	18	36
Ditto 21	8	50	24	14	25	2	02	24	23	22	7	02	24	17	40
Ditto 22	8	12	24	15	00	—	—	—	—	—	7	10	24	19	03
Ditto 23	8	40	24	16	14	—	—	—	—	—	—	—	—	—	—
Ditto 25	8	48	24	15	50	2	10	24	23	05	—	—	—	—	—
Ditto 27	8	40	24	14	20	1	58	24	22	45	7	22	24	16	16
Ditto 28	8	45	24	16	10	—	—	—	—	—	—	—	—	—	—
Ditto 29	8	50	24	15	00	1	52	24	24	13	6	52	24	13	38
Ditto 30	8	55	24	14	46	1	55	24	24	43	7	07	24	18	35
Ditto 31	8	45	24	16	40	1	53	24	24	26	7	25	24	16	38

Mean of Observations in July.	Morning	at	8h 37′…..	Variation	24°	14′	32″	
	Noon	at	1 50…..	Ditto	24	23	04	
	Evening	at	7 08…..	Ditto	24	~~13~~	~~56~~	16.4
Ditto in June.	Morning	at	8 30…..	Ditto	24	12	55	
	Noon	at	1 33…..	Ditto	24	22	17	
	Evening	at	7 04…..	Ditto	24	16	04	
Ditto in May.	Morning	at	8 28…..	Ditto	24	12	02	
	Noon	at	1 37…..	Ditto	24	20	54	
	Evening	at	6 14…..	Ditto	24	13	47	
Ditto in April	Morning	at	8 31…..	Ditto	24	09	18	
	Noon	at	0 59…..	Ditto	24	21	12	
	Evening	at	5 46…..	Ditto	24	15	25	

Magnetical Observations continued.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
Aug. 1	8h	40′	24°	27′	12″	1h	50′	24°	24′	16″	7h	15′	24°	16′	49″
Ditto 2	9	47	24	16	33	—	—	—	—	—	7	25	24	16	08
Ditto 3	8	10	24	15	36	1	50	24	22	57	7	40	24	18	18
Ditto 4	8	40	24	14	02	1	55	24	22	21	7	45	24	16	46
Ditto 5	9	00	24	18	32	1	57	24	23	25	7	35	24	16	12
Ditto 6	8	02	24	16	31	—	—	—	—	—	—	—	—	—	—
Ditto 7	8	37	24	16	04	—	—	—	—	—	7	20	24	16	54
Ditto 8	9	05	24	15	47	1	53	24	22	30	7	37	24	16	56
Ditto 9	7	50	24	14	39	—	—	—	—	—	—	—	—	—	—
Ditto 10	8	55	24	18	20	2	15	24	23	30	7	20	24	14	45
Ditto 11	8	42	24	17	36	2	00	24	23	34	7	07	24	17	10
Ditto 12	8	45	24	15	34	2	10	24	21	48	7	15	24	14	35
Ditto 13	8	~~35~~ 55	24	16	44	1	55	24	24	17	7	35	24	15	27
Ditto 14	8	45	24	17	55	—	—	—	—	—	6	30	24	16	04
Ditto 15	8	45	24	16	26	2	02	24	24	25	7	18	24	15	07
Ditto 16	8	30	24	14	32	—	—	—	—	—	6	50	24	16	32
Ditto 17	8	35	24	14	15	1	55	24	23	46	7	00	24	13	27
Ditto 18	8	30	24	13	26	—	—	—	—	—	—	—	—	—	—

August 1.—The variation is very remarkable. The morning variation of both needles gave within 18 seconds. The noon variation was less than either morning or evening. The weather was calm, no thunder, nor any apparent cause for so singular a circumstance. At noon it was very gloomy, with the appearance of a blight. Greatest height of the thermometer, 68.

Rain { Between noon of the 1st July / Between noon of the 1st Aug. } 2·176 inches.

ERRATA IN THE LAST NUMBER, ARTICLE XII.

Line 18, read b = 25·07.
Line 18, read d = 25·67.
Page 139, line 3, for 1st, 2d, 3d, and 4th, read 1st, 2d, 4th, and 5th.
Page 139, line 12, for 1st, 2d, 3d, and 4th wires, read 1st, 2d, 4th, and 5th wires: and, for Arion's belt, read Orion's belt.

ARTICLE XI.

ANALYSES OF BOOKS.

Philosophical Transactions for 1813. Part I.] This part contains the following papers; of all of which some account has been given in the first volume of the *Annals of Philosophy*; but we consider it as necessary to give an analysis of them here, in order to correct the mistakes, and supply the deficiencies, of our former accounts.

I. *On a new detonating Compound.* By Sir Humphry Davy, LL.D. F.R.S.] This is the substance of which an analysis by Sir H. Davy is given in our seventh number under the name of *azotane.* We now know that the original discoverer of it was a M. Dulong, and that he obtained it by passing a mixture of chlorine and azotic gas through a solution of sal ammoniac. He has lately published a paper on the subject in the *Annales de Chimie*; but it contains nothing that we did not know before. Azotane has the colour and fluidity of olive oil; its specific gravity is above 1·6. It detonates with a bright light when heated in water, and when brought in contact of oil or phosphorus: it acts slowly on ether and alcohol: with mercury it forms corrosive sublimate; on tin and zinc it has no action: neither does it act on sulphur or resin: it explodes in alcohol and ether, having phosphorus in solution: in muriatic acid it gives off gas rapidly, but does not explode: it has but little action on sulphuric acid. Sir H. Davy conceives it probable, from the properties and formation of this substance, that it is a compound of chlorine and azote; an opinion which he has since verified by a direct analysis.

II. *On a remarkable Application of Cotes's Theorem.* By I. F. W. Herschell, Esq.] This paper, being entirely algebraical, and not susceptible of abridgement, could scarcely be made intelligible to the reader without transcribing the whole of it. On that account I must refer those who are curious to know the application of Mr. Herschell to the paper itself. I may only observe, that the conclusions of the ingenious author apply rather to the algebraic expressions than to the conic sections themselves.

III. *Observation of the Summer Solstice* 1812, *at the Royal Observatory.* By John Pond, Esq. Astronomer Royal, F.R.S.] From this observation, made with the new mural circle, with more accuracy, Mr. Pond informs us, than any former instrument was susceptible of, the obliquity of the ecliptic comes out 23° 27′ 52·25″.

IV. *Observations relative to the Near and Distant Sight of different Persons.* By James Ware, Esq. F.R.S.] This curious paper contains a great many cases of near-sighted persons, with remarkable changes produced in the sight by different causes. These cases authorize the following conclusions:—

1. Near-sightedness is rarely observed in infants, or even in children under ten years of age. It affects the higher classes of society more than the lower: and the instances are few, if any, in which, if the use of concave glasses has been adopted, increasing years have either removed or lessened this imperfection.

2. Though the usual effect of time on perfect eyes be that of inducing a necessity to make use of convex glasses, in order to see near objects distinctly, yet sometimes, even after the age of fifty, and after convex glasses have been used many years for this purpose, the eyes have not only ceased to derive benefit from them, when looking at near objects, but they have required concave glasses to enable them to distinguish with precision objects at a distance.

3. Though the cause of this change be not always known, yet sometimes it has been induced by the use of evacuating remedies, particularly of leeches applied to the temples; and sometimes by looking through a microscope, for a continued length of time, for several successive days.

4. Instances are not uncommon in which persons far advanced in life (viz. between eighty and ninety), whose eyes have been accustomed for a long time to the use of deeply convex glasses, when they have read or written, have ceased to derive benefit from these glasses, and they have become able, without any assistance, to see both near and distant objects almost as well as when they were young. Although it be not easy to ascertain the cause of this amended vision, it seems not improbable that it is occasioned by an absorption of part of the vitreous humour; in

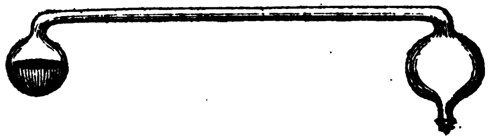
consequence of which the sides of the eye collapse, and its axis from the cornea to the retina is lengthened; by which alteration the length of this axis is brought into the same proportion to the flattened state of the cornea, or crystalline, or both, which it had to these parts before the alteration took place.

V. *The Bakerian Lecture. On the Elementary Particles of certain Crystals.* By William Hyde Wollaston, M.D. Sec. R.S.] The regular octahedron is well known to crystallographers to constitute the primitive form of a considerable number of minerals. Now there is a difficulty with respect to the figure of the integrant molecules of such bodies. The octahedron may be divided into octahedrons and tetrahedrons, and the same observation applies to each of the tetrahedrons thus obtained; so that two different figures present themselves. If we make choice of the tetrahedron, we must suppose the body full of octahedral spaces; and if we adopt the octahedron, the body must be full of tetrahedral spaces: and the molecules, whether octahedral or tetrahedral, must unite by their edges instead of their faces. These suppositions, it must be obvious, are of such a nature as not to be satisfactory. Dr. Wollaston gets over the difficulty by supposing the integrant molecules in such cases to be spherical: and he shows how spherical molecules are capable of forming tetrahedrons, octahedrons, and rhomboids. He shows, also, that rhomboids would be the result of the union of oblate spheroids. Oblong spheroids, on the contrary, would form three and six-sided prisms. He shows how cubes might be the result of the union of two sets of spherical particles, each set forming a tetrahedron.

VI. *On a Substance from the Elm-tree called Ulmin.* By James Smithson, Esq. F.R.S.] The substance to which I gave the name of ulmin was examined by Klaproth about a dozen of years ago, and the account of its properties which I gave in my *System of Chemistry* was taken without alteration from Klaproth's paper. More than two years ago, however, I had satisfied myself, by the examination of some ulmin from Plymouth, that Klaproth's account was in several respects inaccurate, and that it had been the result of a very hasty and imperfect examination of the substance in question. My experiments were published in the first number of the *Annals of Philosophy*. Mr. Smithson's experiments, having been made upon a portion of the same Palermo ulmin which Klaproth examined, remove every doubt respecting the properties of this peculiar substance. Palermo ulmin, in lumps, appears black; but in thin pieces it is transparent, and of a deep red colour. It dissolves readily in water; the dilute solution is yellow, the concentrated, the colour of blood. This solution appears to be a combination of potash with a peculiar substance, to which the name of ulmin may be given

The ulmin is precipitated by nitric, sulphuric, muriatic, phosphoric, oxalic, tartaric, and citric acids; but not by distilled vinegar. Ulmin thus precipitated is glossy, and has a resinous appearance. Its colour is unaltered. It burns with flame, and is reduced to white ashes. Alcohol and water dissolve it, but only in very small quantity. Potash promotes its solution in water; but neither ammonia nor carbonate of soda produce this effect. English ulmin examined by Mr. Smithson exhibited similar properties; but the quantity was too small to enable him to ascertain all the properties which I detailed in my paper.

VII. *On a Method of Freezing at a Distance.* By William Hyde Wollaston, M.D. Sec. R.S.] It is well known that the temperature of liquids is cooled by evaporation; but in close vessels evaporation is limited by the great bulk into which the vapour expands. Dr. Wollaston in this paper describes an ingenious contrivance of his, to which he has given the name of *cryophorus*, by means of which water may be frozen in close vessels by its evaporation with great facility. This instrument is a glass tube of this shape—



Its internal diameter is about $\frac{1}{8}$th of an inch, each extremity is blown into a ball, and the tube is bent at right angles about half an inch from each ball. One of the balls is filled not quite half full of water. This liquid is boiled for some time to expel the air, and the capillary tube at the extremity of the other ball is then sealed hermetically. If the empty ball of this instrument is plunged into a mixture of snow and salt, the vapour within it is condensed so fast that the water in the other ball freezes.

VIII. *A Catalogue of North Polar Distances of some of the principal Fixed Stars.* By John Pond, Esq. Astronomer Royal, F.R.S.] This catalogue contains 44 stars, the distances of all of which are determined within rather less than a second of absolute precision.

IX. *A Description of the Solvent Glands and Gizzards of the Ardea Argala, the Casuarius Emu, and the Long-legged Cassowary, from New South Wales.* By Sir Everard Home, Bart. F.R.S.] The ardea argala, a native of Bengal, feeding on carrion, and very voracious, has its solvent glands disposed in two circular masses, one on the anterior, the other on the posterior side of the cardiac cavity. Each gland is made up of five or six cells, and these open into one common excretory duct. In the casuarius emu the solvent glands are oval bags $\frac{1}{4}$th of an inch long and $\frac{1}{16}$th of an inch wide. They occupy the whole surface of the cardiac cavity, which is very large. The gizzard is situated out of the direction of the cardiac cavity, so that the food can pass into the duodenum without entering the gizzard. The structure of these organs in the long-legged cassowary of New South Wales is similar. Sir Everard Home terminates his paper by showing that the length of the intestines in birds increases with the scarcity of their food.

In the Casuarius Emu the intestines are 7 feet long.
Rhea Americana ....19 feet 11 inches.
Struthio Camelus ....77 feet 9 inches.

X. *Additional Remarks on the State in which Alcohol exists in Fermented Liquors.* By William Thomas Brande, Esq. F.R.S.] To confirm his former conclusions respecting the great quantity of alcohol contained in wine, Mr. Brande had recourse to the following process. He dropped into the wine a solution of acetate of lead (Goulard's extract) till the whole colouring matter was precipitated. The wine thus freed from colouring matter was mixed with dry subcarbonate of potash, and set aside. Alcohol soon separated, and floated on the surface of the liquid. The quantity of alcohol thus obtained agreed with his former experiments nearly. He found that when Port wine was partly distilled, the portion in the receiver being mixed with that in the alembic, the wine was restored to its original taste and specific gravity. Mr. Brande now states the quantity of alcohol of 0·825 in 100 measures of Port wine at 22 measures.

XI. *On a new Variety in the Breeds of Sheep.* By Colonel David Humpries, F.R.S.] Seth Wight, who possessed a small farm on the Charles River, about 16 miles from Boston, had a small flock, consisting of 15 ewes and one ram. One of these ewes in 1791 produced a singular shaped lamb. He was advised to kill his former ram, and keep this new one in place of it: the consequence was, the formation of a new breed of sheep, which gradually spread over a considerable part of New England: but the introduction of the merinos has nearly destroyed them again. This new variety was called the *otter* or *ankon* breed. They are remarkable for the shortness of their legs, and the crookedness of their fore legs, like an elbow. They are much feebler, and much smaller, than the common sheep, and less able to break over low fences. This was the reason of their being continued and propagated.

XII. *Experiments to ascertain the coagulating Power of the Secretion of the Gastric Glands.* By Sir Everard Home, Bart. F.R.S.] The method of making these experiments was to bring the different substances examined in contact with milk, and observe whether they coagulated that liquid. The results were as follows:—

The mucus of the fourth cavity of a calf's stomach coagulated milk, but not the mucus of the first, second, and third cavity. After a calf has left off sucking, the inner member of the fourth cavity of the stomach still coagulates milk. Only that part of a hog's stomach near the pylorus, where the gastric glands are situated, coagulates milk. The crop and gizzard of a cock coagulated milk, the latter more speedily than the former. The contents of a shark's stomach, and the stomach itself, coagulated milk immediately. Rennet from the stomach of the salmon and thornback coagulated milk. From other experiments, Sir Everard Home concluded, that the secretion of the gastric glands possesses the power of coagulating milk, and that it gives that power to all the parts by which it is imbibed, whether composed of living parts or not.

XIII. *On some Properties of Light.* By David Brewster, LL.D. F.R.S. Edin.] These properties are divided into four sets: 1. *On a new Property of Refracted Light.* Thin plates of agate, calcedony, and carnelian, gave a distant image of any luminous object; but on each side of this image was one highly coloured, forming with it an angle of several degrees, and so deeply affected with colour that no prism of agate, with the largest refracting angle, could produce an equivalent dispersion. The light which constituted all these images was polarized. The effect of two plates of agate is similar to what was observed by Biot. There is a faint nebulous light, unconnected with the image, but always accompanying it. This Dr. Brewster conjectures may be owing to an imperfect double refractive power in the agate. 2. *On the double refractive Power of Chromate of Lead.* The refractive power of this substance is thrice as great as that of Iceland spar. 3. *On Substances with a higher refractive Power than the Diamond.* The following is the refractive and dispersive power of different substances, according to Dr. Brewster:—

Refractive Powers.

Chromate of lead, greatest refraction	2·926
Ditto, least refraction	2·479
Realgar	2·510
Diamond, according to Newton	2·439
Phosphorus	2·224
Sulphur, native	2·115
Cryolite	1·344
Ice	1·307

Dispersive Powers.

Chromate of lead, greatest	0·400
Ditto, least refraction	0·262
Realgar	0·255
Oil of cassia	0·139
Sulphur	0·130
Phosphorus	0·128
Flint glass (highest)	0·052
Diamond	0·038
Water	0·035
Fluor spar	0·022
Cryolite	0·022

4. *On the Existence of two dispersive Powers in all doubly refracting Crystals.* When substances refract doubly, one image is always more coloured than the other. Hence it is obvious that such substances possess a doubly dispersive power. The following table exhibits the double dispersions of various bodies:—

Chromate of lead, greatest	0·400
Ditto, least	0·262
Carbonate of lead, greatest	0·091
Ditto, least	0·066
Carbonate of strontian, greatest	0·046
Ditto, least	0·027
Calcareous spar, greatest	0·040
Ditto, least	0·026

XIV. *An Appendix to Mr. Ware's Paper on Vision.* By Sir Charles Blagden, F.R.S.] Sir Charles states his concurrence in opinion with Mr. Ware, that near-sightedness comes on at an early age, and that it is almost confined to the higher ranks. He conceives it to be owing to the habit acquired by such young people of confining their attention to near objects. He describes his own case as an illustration. At the age of four, when he learned to read, he was not near-sighted; but being much addicted to reading, he became slightly so at eight. He was dissuaded from using a glass, and struggled on with a certain degree of near-sightedness till the age of thirty. He then used the glass called No. 2, which in a few years he laid aside for No. 3, then for No. 5, which he still employs.

XV. *A Method of drawing extremely fine Wires.* By William Hyde Wollaston, M.D. Sec. R.S.] The method employed by this ingenious philosopher was to take a small platinum wire, put it into the centre of a mould, and fill the mould with fine silver. The silver is then to be drawn into a wire as fine as thought requisite. The silver wire thus obtained is bent into the form of an U, making two hooks at its upper extremities; and in this position it is dipped for a few minutes into hot nitric acid. The silver is melted off, except at the extremities, and the platinum wire remains. The extreme books, retaining their silver and size, serve to make the platinum wire visible and tangible. By this process he obtained wires only $\frac{1}{18000}$th of an inch in diameter.

XVI. *Description of a single lens Micrometer.* By William Hyde Wollaston, M.D. Sec. R.S.] This instrument possesses the characteristic simplicity and precision which mark all Dr. Wollaston's inventions. It resembles a common telescope with three sliding tubes. Instead of the object-glass is placed a scale of wires, each $\frac{1}{50}$th of an inch in diameter, and easily counted by means of a regular variation in their length. Instead of the eye-glass is a small single lens, with a focus of about $\frac{1}{12}$th of an inch. A small perforation is made by the side of this lens within $\frac{1}{25}$th of an inch of its centre. The substance to be measured is inclosed between two flat glasses, which slide in before the lens, sufficiently near to enable the object to be seen distinctly. The wire, or other substance, to be measured, is seen through the lens, and the scale through the perforation, by the naked eye, and it is seen how many divisions of the scale it covers. In Dr. Wollaston's micrometer one division of the scale corresponds to $\frac{1}{10000}$th of an inch, when it is at the distance of 16·6 inches. Hence at the distance of 8·3 inches it will correspond to $\frac{1}{5000}$th of an inch. Upon the side of the micrometer are marked the value of one division, according to the distance.

XVII. *Observations of the Winter Solstice of* 1812, *with the Mural Circle of Greenwich.* By John Pond, Esq. Astronomer Royal, F.R.S.] From the observations on the summer solstice, corrected by subsequent observations, Mr. Pond deduced the obliquity of the ecliptic 23° 27′ 51·50″: from the winter solstice he deduced it 23° 27′ 47·35″. He thinks it not unlikely that this small discordance may be owing to some slight error in Bradley's refractions; and he is now employed in endeavouring to ascertain whether Bradley's mean refraction does or does not require alteration.

XVIII. *On the Tusks of the Narwal.* By Sir Everard Home, Bart. F.R.S.] Sir Everard had been informed by Mr. Scoresby, jun. that the female narwal had no tusks; and, in consequence of a scull which he received from that Gentleman, he wrote the paper on the subject, an account of which appeared in a former Number of the *Annals of Philosophy*. Subsequent information led him to doubt the accuracy of his information, that the female narwal has no tusks. Mr. Brown having examined all the books on the subject in Sir Joseph Banks's library, found an account of a female narwal with two tusks, brought to Hamburgh, in 1684, by Dick Peterson, and still to be seen there.

On examining the narwal sculls in the Hunterian Museum he found, besides the tusk which had protruded, another milk tusk, seven or eight inches long, and all enclosed in the scull. The scull of the female narwal exhibited two milk tusks in the same situation. Thus it appears that the narwal, both male and female, has two tusks; that one is protruded much earlier than the other, and that the tusks of the male narwal are protruded at a more early period than those of the female.

Article XII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

Medical School of St. Thomas's and Guy's Hospitals.—The winter course of lectures at these adjoining hospitals will commence the beginning of October, viz.:—

At St. Thomas's.—Anatomy and Operations of Surgery; by Mr. Astley Cooper and Mr. Henry Cline.—Principles and Practice of Surgery; by Mr. Astley Cooper.

At Guy's.—Practice of Medicine; by Dr. Babington and Dr. Curry.—Chemistry; by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy; by Mr. Allen.—Theory of Medicine, and Materia Medica; by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children; by Dr. Haighton.—Physiology, or Laws of the Animal Economy; by Dr. Haighton.—Structure and Diseases of the Teeth; by Mr. Fox.

N.B. These several Lectures, with those on Anatomy, and on the Principles and Practice of Surgery, given at the Theatre of St. Thomas's Hospital adjoining, are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt from Mr. Stocker, Apothecary to Guy's Hospital.

Dr. Clarke and Mr. Clarke will begin their Winter Course of Lectures on Midwifery, and the Diseases of Women and Children, on Monday, October 4th. The Lectures are read at the house of Mr. Clarke, No. 10, Upper John-street, Golden-square, from a quarter past ten o'clock till a quarter past eleven, for the convenience of Students attending the Hospitals.

Dr. Roget will commence his Autumnal Course of Lectures on the Practice of Physic at the Theatre of Anatomy, Great Windmill-street, on the first Monday in October.

Mr. Thomas Shute will commence his Winter Course of Lectures on Anatomy, Physiology, and the Principles of, and Operations in, Surgery, at the Anatomical Theatre, Bristol, on the morning of Friday, October 1st, at eight o'clock.

Trinity College, Dublin.—Lectures upon Anatomy, Physiology, and Surgery; by James Macartney, M.D. F.R.S. M.R.I.A. Professor of Anatomy and Surgery in the University, and late Lecturer upon Comparative Anatomy and Physiology at St. Bartholomew's Hospital, London.

These Lectures will be divided into three courses:—1st. A preliminary Lecture of General Physiology, which will be open to the public. 2d. A strict course of Anatomy and Physiology, as applicable to Medical Science. 3d. A course of Surgical Anatomy and Operative Surgery.

The Lectures will commence on the first Monday in November, and terminate at the end of the following April.

Demonstrations will be given daily in the Dissecting Room by Mr. Cross, Member of the Royal College of Surgeons in London.

It is intended to establish regular examinations in the Anatomical Class, and to grant Certificates and Premiums according to merit.

II. *Vinegar from Wood.*

It is well known that when wood is distilled, an acid liquor is obtained, having a strong empyreumatic taste and smell. To this liquor the French chemists gave the name of *pyrolignous* acid; and Fourcroy and Vauquelin, having examined it about twelve years ago, determined that it was not a peculiar acid, but merely vinegar impregnated with an empyreumatic oil. This discovery was not so new as the French chemists conceived. Glauber, in his *Miraculum Mundi*, describes a method of making charcoal and preserving this acid liquid, quite similar to that now practised by several of our gunpowder manufacturers, who were probably obliged to that old chemical writer for the first hint of that improvement. Glauber expressly calls the liquor obtained by distilling wood *vinegar*. (See Glauber's Works, p. 188, English Translation.) This opinion prevailed universally among chemists till it was rejected by the French chemists, without any reason. The experiments of Fourcroy and Vauquelin, instead of being a new discovery, merely corrected the mistake of their countrymen, and restored the old and true opinion respecting the acid from wood, which had formerly been universally prevalent.

III. *Matrix of the Diamond.*

Diamonds have hitherto been found only in India and Brazil. They usually occur in an alluvial soil, from which they are separated by washing and picking. Hence mineralogists have been hitherto ignorant of the true repository of diamonds. Werner, indeed, conjectured that they occurred in the rocks of the newest floetz trap formation; but this conjecture was not supported by any direct proof. Dr. Benjamin Heyné, botanist and naturalist to the East India Company at Madras, has lately brought over to London a piece of the diamond bed at Banaganpally, in the Dekan, with a diamond actually imbedded in it, from which we are enabled to determine the nature of the rock with tolerable accuracy. As it is perhaps the first specimen of the kind brought to Europe, mineralogists will doubtless consider it as of some importance to present them with a description of it. To the eye it has the appearance of a *conglomerate*; but as the grains are commonly roundish, and the cement a clay, approaching to wacke in its appearance and nature, it seems rather entitled to the name of amygdaloid. The round grains of which it is composed are chiefly of chalcedony, of a bluish grey colour, and approaching a little to hyalite in their appearance. They vary in size, from a pin head to a hazel nut. These nodules are mixed with angular fragments of jasper, hornstone, and quartz. No fragments of corundum were visible; though, it is said, they now and then occur in some of the beds containing diamonds.

From the above description there can be little doubt that the rock in question is an amygdaloid belonging to the newest floetz trap formation. Though the nodules are not absolutely identical with those which occur in the amygdaloid of this country, yet they very nearly agree with it.

From Dr. Heyné's description, it would appear that though this amygdaloidal rock is of some considerable thickness, yet the diamonds are confined to a bed in the centre of it, not more than a foot thick. This bed is distinguished from the rest of the rock by being harder.

IV. *Carbonate of Iron.*

It has been known for some time to mineralogists that the substance called sparry iron ore, and *flos ferri*, is a compound of black oxide of iron and carbonic acid, or a carbonate of iron; and Dr. Wollaston has shown that it crystallizes in a form differing a little from that of the primitive crystal of calcareous spar. It has lately been determined, by the chemical analysis of Descotils, and several other chemists, that the species known by the name of *clay iron-stone*, and so much employed in this country as an iron ore, is in fact a *carbonate of iron* likewise, and differs in nothing from sparry iron ore, except in not being crystallized, and in being mechanically mixed with some silicious matter and some manganese. Alumina is usually present only in a very small proportion, and seems to be in combination with the silica; though in some specimens the proportion of lumina is considerable. The following is the result of an nalysis of a specimen of clay iron-stone from Colnbrookdale :—

Silica	10·6
Alumina	2·0
Lime	1·6
Magnesia	2·4
Oxide of iron	50·0
Oxide of manganese	2·6
Loss by calcination	32·0
	101·2

See an essay on this species of ore, by Descotils, in the *Annales de Chimie*, lxxxiv. 188.

V. *Peroxide of Lead.*

A phenomenon lately occurred to Chevreul which deserves to be recorded. He was employed in analysing a specimen of crystal glass, and for that purpose fused it in a platinum crucible with potash. When the process was finished, he found in the crucible an alloy of lead and platinum, and a quantity of peroxide of lead, mixed with the other ingredients in the fused mass.

VI. *Subsulphate of Alumina.*

In Vol. I. p. 467, of the *Annals of Philosophy*, mention was made of specimens of pure alumina found by Mr. Webster between Brighton and Beachy Head. Specimens have been since brought to London by Mr. Tennant, and the substance has been more carefully examined by Dr. Wollaston and Mr. Smithson, and both have found it to be *not pure alumina*, but a subsulphate of alumina.

ARTICLE XIII.

Scientific Books in hand, or in the Press.

Dr. Hodgson will publish, in October, a Treatise on the Diseases of Arteries and Veins, comprising the Pathology and Treatment of Aneurisms and Wounded Arteries. The Work will consist of One Volume, 8vo, illustrated by a Series of Engravings in 4to.

Dr. Hutton has nearly ready for publication a Second Edition of Mathematics and Natural Philosophy, in Four 8vo Plates.

ARTICLE XIV.

METEOROLOGICAL JOURNAL.

		BAROMETER.			THERMOMETER.					
1813.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Rain.	
7th Mo.										
July 20	E	29·51	29·49	29·500	75	56	65·5	—		☾
21	N W	29·55	29·51	29·530	68	57	62·5	—		
22	S W	29·55	29·50	29·525	75	58	66·5	—		
23	S W	29·50	29·42	29·460	75	53	64·0	—	1·00	
24	W	29·53	29·40	29·465	74	55	64·5	·48	·77	
25	S W	29·54	29·40	29·470	73	53	63·0	—	4	
26	W	29·78	29·54	29·660	69	53	61·0	—	·32	
27	W	30·07	29·78	29·925	75	51	63·0	·32	·10	●
28	W	30·17	30·07	30·120	77	50	63·5	—		
29	E	30·07	29·95	30·010	78	54	66·0	—		
30	S	29·91	29·80	29·855	82	60	71·0	·56		
31	N W	29·95	29·95	29·930	78	53	65·5	—		
8th Mo.										
Aug. 1	S W	29·96	29·93	29·945	78	59	68·5	—	5	
2	W	29·93	29·81	29·870	79	56	67·5	—		
3	S W	29·78	29·72	29·750	76	51	63·5	·38	—	
4	N W	29·75	29·55	29·650	73	49	61·0	—	—	☾
5	S	29·53	29·42	29·475	71	49	60·0	—	·26	
6	N W	29·85	29·53	29·690	73	54	63·5	—	1	
7	W	29·92	29·91	29·915	74	49	61·5	—		
8	S W	29·89	29·87	29·880	75	53	64·0	·50	6	
9	N W	30·10	29·89	29·995	70	49	59·5	—	—	
10	Var.	30·10	30·07	30·085	76	57	66·5	—	—	
11	S E	30·07	29·98	30·025	79	51	65·0	·30		
12	S W	29·96	29·89	29·925	80	54	67·0	—		○
13	N W	30·08	29·95	30·015	72	48	60·0	—	—	
14	N W	30·08	29·83	29·955	72	57	64·5	·42	—	
15	N W	29·83	29·78	29·805	76	51	63·5	—		
16	N W	29·82	29·80	29·810	70	50	60·0	—		
17	W	29·82	29·78	29·800	76	52	64·0	—		
18	N W	30·05	29·82	29·935	75	47	61·0	·46		
		30·17	29·40	29·799	82	47	63·88	3·42	2·61	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Seventh Month.—23. Continued heavy rain for above two hours, p. m. with distant thunder. 24. A thunder shower about three p. m. Rainbow. 25. Windy: cloudy: rainbow. Broad diverging shadows on a coloured twilight, with *Cirrostratus* and haze to the S. 26. Fair, a. m. At noon began a steady rain, with distant thunder. In the evening several distinct *Nimbi*; in particular, a well formed one in the N. E. 27. a. m. *Cumulostratus*; p. m. rain, with distant thunder. In the evening distinct *Nimbi*, and a rainbow: much colour in the twilight, with broad shadows on it. 28. Much dew: *Cumulus*, with *Cirrus*. At sunset a calm air, with large plumon *Cirri*, highly coloured. 29. A clear day: the wind passed from S. to E. Twilight brilliant, with dew: the new moon showed a well defined disc. 30. *Cumulus*, with *Cirrus* passing to the inferior modifications. In the evening there was the addition of haze, with a pink colour in the twilight. It lightned frequently before ten at night very far in the S. E. with the wind S.

Eighth Month.—1. Rain at five a. m.: succeeded by a close canopy of *Cumulostratus*. At sunset, *Cirrus*, with *Cirrocumulus*: twilight opake, somewhat orange coloured. 2. Much the same phenomena as yesterday. 3. Some drizzling showers, with wind, a. m. Sunset very dark, the sky being full of low broken *Cumuli*: night windy. 4. a. m. Windy, with *Cumulus*, which p. m. inosculated with *Cirrostratus* above it. 5. Rain early; the wind S. In the evening (after several showers) clouds in various modifications, the wind W. with lightning to the S. 6. Much wind at N. W., with *Cumulus*: a shower, p. m. 8. Close *Cumulostratus* most of the day. Rain, evening. 9. Wind brisk at N.N.W. a. m. At noon the upper clouds were perceived not to move with this wind: at evening it fell calm: there were now in the sky rose-coloured *Cirri*, in stripes from S. E. to N.W. with *Cirrostratus* and *Cumulostratus* in a lower region: twilight orange, surmounted with rose-colour. 11. A *Stratus* after sunset, with *Cirrostratus* remaining above. Small scintillant meteors now appeared, falling almost directly down, and seeming to originate very low in the atmosphere. 13. *Cirrus* and *Cirrocumulus* abounded. There was a slight shower about noon. 14. Overcast: a little rain after sunset. 16. The *maximum* of temperature, for 24 hours past, occurred at 9 a. m. 17. Overcast: windy.

RESULTS.

Prevailing winds, Westerly.

Barometer: greatest height ....30·17 inches;
Least ....29·40 inches;
Mean of the period ......29·799 inches.

Thermometer: greatest height ....82°
Least ....47°
Mean of the period....63·88°

Evaporation, 3·42 inches. Rain, 2·61 inches.

Tottenham, Eighth Month, 25, 1813. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

OCTOBER, 1813.

ARTICLE I.

Biographical Account of Sir Isaac Newton.[*]

SIR ISAAC NEWTON was born on Christmas-day, old style, 1642,[†] at Woolsthorpe, in the parish of Colsterworth, in the county of Lincoln, near three months after the death of his father, who was descended from the eldest branch of the family of Sir John Newton, Bart. and was Lord of the Manor of Woolsthorpe. The family came originally from Newtown, in the county of Lancaster, from which probably they took their name. His mother was Hannah Ayscough, of an ancient and honourable family in the county of Lincoln. She was married a second time to the Rev. Barnabas Smith, Rector of North Witham, and had by him a son and two daughters, from whom were descended the four nephews and nieces, who inherited Sir Isaac's personal estate. Sir Isaac went to two little day schools at Skillington and Stoke, till he was twelve years old, when he was sent to the great school at Grantham, under Mr. Stokes, who had the character of being a very good schoolmaster. While at Grantham, he boarded in the house of Mr. Clark, an apothecary, whose brother was at that time usher of the school.

Here he soon gave proofs of a surprising genius, and astonished his acquaintances by his mechanical contrivances. Instead of playing among other boys, he always busied himself in making curiosities, and models of wood of different kinds. For this purpose he had got little saws, hatchets, hammers, and all sorts of tools, which he knew how to use with great dexterity. He even went so far as to make a wooden clock. A new windmill was set up about this time near Grantham, in the way to Gunnerby. Young Newton's imitating genius was excited; and by frequently prying into the fabric of it, as they were making it, he contrived to make a very perfect model, which was considered at least equal to the workmanship of the original. This sometimes he set upon the house-top where he lodged, and clothing it with sails the wind readily turned it. He put a mouse into this machine, which he called his *miller*; and he contrived matters so that the mouse would turn round the mill whenever he thought proper. He used to joke too about the miller eating the corn that was put into the mill. Another of his contrivances was a water clock, which he made out of a box that he begged from the brother of his landlord's wife. It was about four feet in height, and of a proportional breadth. There was a dial plate at top, with figures for the hours. The index was turned by a piece of wood which either fell or rose by water dropping. This stood in the room where he lay, and he took care every morning to supply it with its proper quantity of water.

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[*] From Dr. Thomson's History of the Royal Society, p. 277.

[†] When born he was so little that his mother used to say that he might have been put into a quart mug; and so unlikely to live, that two women, who were sent to Lady Pakenham's, at North Witham, for something for him, did not expect to find him alive at their return.

These fancies sometimes engrossed so much of his thoughts, that he was apt to neglect his book, and dull boys were now and then put over him in his form. But this made him redouble his pains to overtake them; and such was his capacity that he could soon do it, and outstrip them when he pleased; and this was taken notice of by his master.* Still nothing could induce him to lay aside his mechanical inventions; but during holidays, and every moment allotted to play, he employed himself in knocking and hammering in his lodging room, pursuing the strong bent of his inclination, not only in things serious, but likewise in ludicrous contrivances, calculated to please his schoolfellows as well as himself: as, for example, paper kites, which he first introduced at Grantham. He took pains to find out their proper proportion and figures, and the proper place for fixing the string to them. He made lanterns of paper crimpled, which he used to go to school by in winter mornings with a candle, and he tied them to the tails of his kites in a dark night, which at first frightened the country people exceedingly, who took his candles for comets. He was no less diligent in observing the motion of the sun, especially in the yard of the house where he lived, against the wall and roof, wherein he drove pegs to mark the hours and half hours made by the shade. These, by some years' observation, he made very exact, so that any body knew what o'clock it was by Isaac's dial, as they usually called it.

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* Sir Isaac used to relate that he was very negligent at school, and very low in it, till the boy above him gave him a kick on the belly, which put him to great pain. Not content with having thrashed his adversary, Sir Isaac could not rest till he had got before him in the school; and from that time he continued rising till he was the head boy.

His turn for drawing, which he acquired without any assistance, was equally remarkable with his mechanical inventions. He filled his whole room with pictures of his own making; copied partly from prints, and partly from the life. Among others, were portraits of several of the kings, of Dr. Donne, and of Mr. Stokes, his school-master.*

Mrs. Vincent was niece to the wife of Sir Isaac's landlord, at Grantham, and lived with him in the same house. According to her account, he very seldom joined with his school-fellows in their boyish amusements, but chose rather to be at home, even among the girls, and would frequently make little tables, cupboards, and other utensils, for her and her play-fellows to set their babies and trinkets in. She mentioned, likewise, a cart which he made with four wheels, in which he would sit, and by turning a windlass about, make it carry him round the house wherever he pleased. He is said to have contracted an attachment to Mrs. Vincent, whose maiden name was Storey, and would have married her, but being himself a Fellow of a College, with hardly any other income, and she having little or no fortune of her own, he judged it imprudent to enter into any matrimonial connection. But he continued to visit her as long as he lived, after her marriage, and repeatedly supplied her with money when she wanted it.

During all this time the mother of Sir Isaac lived at North Witham, with her second husband; but, upon his death, she returned to Woolsthorp, and, in order to save expenses as much as she could, she recalled her son from school, in order to make him serviceable at Woolsthorp, in managing the farm and country business. Here he was employed in superintending the tillage, grazing, and harvest; and he was frequently sent on Saturdays to Grantham market, with corn and other commodities to sell, and to carry home what necessaries were proper to be bought at a market-town for a family; but, on account of his youth, his mother used to send a trusty old servant along with him, to put him in the way of business. Their inn was at the Saracen's Head, in Westgate, where, as soon as they had put up their horses, Isaac generally left the man to manage the marketing, and, retiring to Mr. Clarke's garret, where he used to lodge, entertained himself with a parcel of old books, till it was time to go home again; or else he would stop by the way, between home and Grantham, and lie under a hedge studying, till the man went to town and did the business, and called upon him on his way back. When at home, if his mother ordered him into the fields to look after the sheep, the corn, or upon any other rural employment, it went on very heavily under his management. His chief delight was to sit under a tree with a book in his hands, or to busy himself with his knife in cutting wood for models of somewhat or other that struck his fancy; or he would get to a stream and make mill-wheels.*

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* He informed his nephew, Mr. Conduitt, that he had a facility in making verses. This is the more remarkable, as he had been heard to express a contempt for poetry. Hence it is probable that the following lines, which he wrote under the portrait of Charles I., were of his own composition. They were given by Dr. Stukely, from Mrs. Vincent, who repeated them from memory.

A secret art my soul requires to try,
If prayers can give me what the wars deny.
Three crowns distinguished here in order do
Present their objects to my knowing view.
Earth's crown, thus at my feet, I can disdain,
Which heavy is, and, at the best, but vain.
But now a crown of thorns I gladly greet:
Sharp is this crown, but not so sharp as sweet,
The crown of glory that I yonder see
Is full of bliss and of eternity.

If Newton wrote these lines, it must be remembered that they were written when he was only a boy at school.

This conduct of her son induced his mother to send him to Grantham school again for nine months; and then to Trinity College, in Cambridge, where he was admitted the 5th of June, 1660. He always informed himself, beforehand, of the books which his tutor intended to read; and when he came to the lectures, he found he knew more of them than his tutor himself. The first books which he read for that purpose, were Saunderson's Logic, and Kepler's Optics. A desire to know whether there was any thing in judicial astrology, first put him upon studying mathematics. He discovered the emptiness of that study as soon as he erected a figure; for which purpose he made use of two or three problems in Euclid, which he turned to by means of an index. He did not then read the rest, looking upon it as a book containing only plain and obvious things. This neglect of the ancient mathematicians, we are told by Dr. Pemberton, he afterwards regretted. The modern books which he read gave his mind, he conceived, a wrong bias, vitiated his taste, and prevented him from attaining that elegance of demonstration which he admired in the ancients. The first mathematical book that he read was Descartes' Geometry; and he made himself master of it by dint of genius and application, without going through the usual steps, or having the assistance of any person. His next book was the Arithmetic of Infinites, by Dr. Wallis. On these books he wrote comments as he read them, and reaped a rich harvest of discoveries, or, more properly, indeed, made almost all his mathematical discoveries as he proceeded in their perusal.

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* The preceding details were collected upon the spot, by Dr. Stukely, Sir Isaac's schoolfellows, and the companions of his boyhood.

In 1664 he bought a prism, as appears by some of his own accounts of expenses at Cambridge, to try some experiments upon Descartes' doctrine of colours, and soon satisfied himself that the hypothesis of Descartes was destitute of foundation. The further prosecution of the subject satisfied him respecting the real nature of light and colours. He soon after drew up an account of his doctrine, which was published in the Philosophical Transactions, and unfortunately gave origin to a controversy between him and some foreign opticians, which produced an unhappy effect upon his mind, and prevented him from publishing his mathematical discoveries, as he had originally intended. He communicated them, however, to Dr. Barrow, who sent an account of them to Collins and Oldenburg, and by that means they came to be known to the Members of the Royal Society.

He laid the foundation of all his discoveries before he was 24 years of age. In the year 1665, when he retired to his own estate on account of the plague, the idea of his system of gravitation first occurred to him, in consequence of seeing an apple fall from a tree. This remarkable apple-tree is still remaining, and is usually shown to strangers as a curiosity. At that time, not being in possession of any accurate measure of the earth's surface, he estimated the force of gravity erroneously, and found, in consequence, that it was not capable alone of retaining the moon in her orbit. This induced him to dismiss his hypothesis, at that time, as erroneous. But afterwards, when Picard had measured a degree of the earth's surface, with tolerable accuracy, he was enabled to make a more precise estimate, and found that the force of gravity exactly accounted for the moon's motion in her orbit. He applied his doctrine to the planets and the whole solar system, and found it to account, in a satisfactory manner, for the whole phenomena of the motions of these bodies.

In 1667 he was elected Fellow of Trinity College, in Cambridge; and, in 1669, Dr. Barrow resigned his Mathematical Professorship to him. In 1671 he was elected Fellow of the Royal Society. It is stated, I do not know upon what authority, that at this time he was so poor that he was obliged to apply to the Society for a dispensation from the usual contribution of a shilling a week, which all the Fellows of the Society regularly pay. His estates (for he had two) were worth about 80*l.* a year.

I do not know the value of his Fellowship, nor of his Professorship, but both together could not be less, surely, than 100*l.* a year; so that his income, at that time, could not well be less than 200*l.* a year. Upon this, indeed, he had his mother and her family to support; but if we consider the expense of living in 1671, we can hardly pronounce Mr. Newton, at that time, a poor man. In 1675 he had a dispensation from King Charles II. for retaining his Fellowship without taking orders. In 1687 he was chosen one of the Delegates to represent the University of Cambridge, before the High Commission Court, to answer for their refusing to admit Father Francis Master of Arts, upon the King's mandamus, without his taking the oaths prescribed by the statutes. He was a great instrument in persuading his colleagues to persist in the maintenance of their rights and privileges. So strenuous, indeed, was the defence which he made, that the Crown thought proper to drop its pretensions. In 1688 he was chosen by the University of Cambridge, Member of the Convention Parliament, and sat in it till its dissolution. He was chosen Member of Parliament also for the same University in 1701. In 1696 the Earl of Halifax, at that time Mr. Montague, and Chancellor of the Exchequer, who was a great patron of the learned, wrote him a letter to Cambridge, acquainting him that he had prevailed with the King to make him Warden of the Mint.* In this post he did signal service in the great re-coinage, which took place soon after. In 1699 he was made Master and Worker of the Mint; in which situation he continued till his death, and behaved himself with an universal character of integrity and disinterestedness. He had frequent opportunities of employing his skill in mathematics and chemistry, particularly in his Table of Assays of Foreign Coins, which is printed at the end of Dr. Arbuthnot's Book of Coins.

In 1701 he made Mr. Whiston his Deputy Professor of Mathematics at Cambridge, and gave him all the salary from that time, though he did not absolutely resign the Professorship till

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* The letter was as follows:—

SIR,

I AM very glad that at last I can give you a good proof of my friendship, and the esteem the King has of your merits. Mr. Overton, the Warden of the Mint, is made one of the Commissioners of the Customs, and the King has promised me to make Mr. Newton Warden of the Mint. The office is the most proper for you; 'tis the chief office in the Mint, 'tis worth five or six hundred pounds per annum, and has not too much business to require more attendance than you may spare. I desire that you will come up as soon as you can, and will take care of your warrant in the mean time. Let me see you as soon as you come to town, that I may carry you to kiss the King's hand. I believe you may have a lodging near me.

I am, &c. CHARLES MONTAGUE.

1703. In the year 1703 he was chosen President of the Royal Society, and continued to fill that honourable situation till the time of his death. In 1705 he was knighted by Queen Anne, at Cambridge.

(To be continued.)

Article II.

Some Mineralogical Observations on Cornwall. By Thomas Thomson, M.D. F.R.S.

(With a Plate of St. Michael's Mount.)

Cornwall has been lately visited by so many mineralogists, and such ample details on its structure have been furnished by the publications of M. Deluc, Dr. Berger, and Mr. Allan, that perhaps any thing farther on the subject may seem superfluous. I travelled through it with such rapidity, and made my observations in such a hasty and imperfect manner, that I ought to apologize for presuming to lay them before the public: but as some of the conclusions which I thought myself warranted to draw are different from those drawn by preceding observers, and as the knowledge of the structure of Cornwall constitutes a very material point in the geology of Great Britain, I thought it better to present my notes to the public, even at the hazard of falling into mistakes. They will at least serve to call the attention of future observers to some very material points, and may in some measure facilitate their examination of this interesting but difficult province.

The chalk country, as far as I could observe, terminates ten miles west from Dorchester: but the country between that place and Exeter, though hilly, is so completely covered with grass or corn that it is very difficult to make out its structure. The only rocks which I perceived were a floetz limestone, containing petrifactions and a coarse sandstone, which seemed to continue almost to Exeter. The soil round Exeter is red, very deep, and reckoned one of the richest in England. It has the aspect of sand, but the tenacity of clay, and seems to be a mixture of both.

On the road that leads north from Exeter, transition rocks may be seen. The road has been sunk in them in some places to the depth of about six feet, so that they stand as a wall on the west side. They consist of alternate beds of greywacke and greywacke slate. These beds are nearly vertical. The slate is shivery, and possesses the usual characters of greywacke slate. The beds of greywacke are few, and the rock itself not very well characterized. Here and there I thought I observed thin beds of flinty slate in these rocks, I visited the manganese mine at Upton Pyne, three miles north from Exeter. The manganese occurs at the depth of 120 feet. The surface, and for a considerable depth, consists of a red coloured gravel: then comes a coarse and very hard sandstone, composed of fragments of flinty slate, quartz, clay-slate, &c. I conceive it to be a transition sandstone, and intimately connected with greywacke. The manganese, as far as I could determine, constituted a bed; but particular circumstances prevented me from being able to ascertain the point in a satisfactory manner.

Whether the transition rocks continue all the way from Exeter to Plymouth, I do not know. The hill immediately to the south of Exeter is thick strewed with flint. To the south of that occur beds of limestone: but from Ivy Bridge to Plymouth the rocks are clay slate. This clay slate I conceive to be transition, because it is continued without interruption to Plymouth, where the clay slate is indisputably transition.

The rocks round Plymouth are so well exposed, and so well characterized, that there can be no hesitation about the class to which they belong. I saw only two kinds of rock, namely, *limestone* and *clay slate*. The limestone rocks form low round-backed hills, which surround the bay; and they are every where wrought for lime, which is carried to considerable distances, and employed as manure. The colours of this limestone are various, blue, red, white, and grey, often mixed irregularly, but sometimes disposed as if in regular beds. Its lustre is glimmering, and seems to proceed from small crystals of calcareous spar scattered through it. The fracture is splintery, with a tendency to the small conchoidal. This description characterizes the most common transition limestone. Hence there can be no hesitation in considering the Plymouth limestone, and all the rocks that alternate with it, as transition.

In most places, as in the rocks between Plymouth and the Dock, and at the north side of Mount Edgecumbe, it seems to dip towards the sea at an angle of about 70°; but on the north face of Mount Wise, under the telegraph, it has the aspect of horizontal strata, varying in thickness from that of a leaf of paper to a foot or more. These apparent strata are distinguished by the alternation of different colours, chiefly red and white.

I was at some pains in examining this limestone, to see whether I could detect any animal remains in it, because Mr. Playfair informs us that he found a shell in it; but all my labour was to no purpose. I found, indeed, the *patella vulgata* mixed with fragments of it; but the shell was recent, and obviously had come from the shore. Indeed, I am not aware that any shells have ever been observed in this kind of transition limestone, the petrifactions which it contains being of a different kind. Hence it is not unlikely that Mr. Playfair may have been deceived by some appearance similar to that of the patella, which had at first almost deceived myself.

The clay slate breaks easily into thin slates of a good size, and is often used for roofing, and even for covering the walls of houses. Its most common colour is dark blue, though sometimes it is purple. Its lustre is glimmering, from specks of mica, which are best seen when you examine the cross fracture of a thick piece. It contains numerous thin beds of quartz, and is often traversed by quartz veins running in all directions. All these characters are well known to belong to transition clay slate; so that they corroborate the conclusions drawn from the limestone, if corroboration be necessary.

In several places I found beds of limestone and clay slate alternating. In going from Plymouth to the Dock, by the new road over the bridge, we pass by a range of limestone rocks, in which two different beds of clay slate may be observed; the first in a low part of the rock. The line of junction appeared to me to run from north to south; but this was probably a deception, from the way in which the two rocks had been quarried. The second bed lies farther west; and the line of junction may be traced for some way from west to east, both rocks dipping to the sea at an angle of about 70°. As we walk west the clay slate disappears; but its place is supplied by a portion of the limestone, which assumes a slaty structure.

Between Plymouth and Grampound, travelling along the road, I observed no other rock but clay slate. At Grampound, clay slate and greywacke alternate; and this alternation continues all the way to Truro. The road between Grampound and Truro is mended with greywacke. About 100 yards east of the fourth milestone from Truro, there is a quarry of greywacke on the south side of the road. The greywacke is remarkably well characterized, and exactly resembles the same rock in the Pentlands near Edinburgh.

I did not find that the Cornish miners gave the name of *killas* to greywacke, as Dr. Berger and Mr. Allan seem to have taken for granted they did. The rock does not occur in any of the mines that I visited; nor could I find that it had been distinguished by any particular name. The term *killas* is applied in Cornwall solely to clay slate, both primitive and transition. If mica slate occurs it will also have the same name; but I did not observe any mica slate in any of the mining districts which I visited.

The town of Truro is built of a stone very much resembling Bath stone. It is a kind of oolite. As this stone is procured from a quarry lieing on the south side of Truro, it must, I conceive, lie among the transition rocks; but as I did not see the quarry, I can give no information respecting it.

The clay slate and greywacke extend as far south as the river Hel, about four miles south of Falmouth. I crossed the mouth of that river in the evening, landed at Portalla, and spent the night in a fisherman's cottage. Next morning the clay slate was no longer to be seen. The rocks at Portalla are porphyry. The base is a compact felspar, approaching somewhat to hornstone in its hardness and fracture. This porphyry continued but a short way as we walked south, and was succeeded by a beautiful and very hard greenstone, interspersed here and there with beds of greenstone porphyry. This greenstone appeared to me to extend south about three miles; but as I went along the sea shore, and followed the windings of the coast, it was not easy to form an accurate conception of the true breadth of the beds over which I passed.

The greenstone is followed by a very beautiful bed of *diallage rock*. This rock has been frequently observed in other countries, and is well known to be intimately connected with serpentine. The Italians have given it the name of *gabbro*; but Von Buch, who met with it in abundance in Norway, has distinguished it by the name of *diallage rock*, a name which I have adopted, as more suitable to our language than *gabbro*. It is composed of compact felspar and diallage. In the peninsula of the Lizzard, the diallage is usually brown, commonly with a shade of grey or green. The felspar is sometimes dark coloured, sometimes white.

The road by the sea-side in this part of the peninsula became so difficult that I was obliged to leave it, and penetrate into the interior of the country. The consequence was, that I missed the termination of the diallage rock, and the commencement of the *serpentine*, the bed which follows next in order; for the interior of this peninsula consists of a flat land, wholly covered with heath, or grass, or corn; so that the rocks are almost every where hid from our view. The serpentine appeared to me to commence between four and five miles north from the Lizzard Point, and it continues to within about half a mile of the light-house. The serpentine of the Lizzard is the common serpentine, and is distinguished by the usual variety of colours of that beautiful rock, but red is by far the most prevalent. On the sea shore it is every where altered on the surface by the action of the weather: it is covered by a thin white crust, and every where split by innumerable rifts, which appeared to me to separate very accurately all the different coloured pieces from each other.

About half a mile from the Lizzard Point the serpentine terminates, and is succeeded by a bed of clay slate, which continues to the sea shore, and constitutes the most southern extremity of Great Britain. This clay slate possesses an unusual proportion of mica; but its characters are those of clay slate, and not of mica slate, as Dr. Berger has called it, misled, I presume, by the quantity of mica which it contains.

All the above described beds, which constitute the peninsula of the Lizzard, lie in a conformable position to each other, and dip to the south at an angle of more than 70°, as is the case with the transition slate on their north side. Hence they all lie on that slate, and if produced north would cover it. We cannot doubt, therefore, that they all belong to the same formation, or that the whole peninsula of the Lizzard consists of transition rocks. The clay slate at its southern extremity, then, is a transition slate; and indeed it possesses all the characters of it. The only circumstance which could have prevented preceding observers from drawing this very obvious conclusion, is the existence of serpentine in the district, which has been hitherto conceived to belong exclusively to primitive rocks; but as the diallage rock, which is so intimately connected with serpentine, and so frequently accompanies it, had been already found as a transition rock, mineralogists were naturally lead to expect that serpentine would occasionally occur in the same position. The occurrence of transition serpentine, then, in Cornwall, is no more than a fact which we were previously led to expect.

The soap rock which occurs at the Lizzard is found in the serpentine. It is said to constitute a vein; but after examining the supposed vein with some attention, I confess that the soap rock did not appear to me to constitute a true vein, but to be merely a portion of the serpentine itself altered by the action of water, or some other cause. It exhibits the same colours as serpentine, only they are all lighter; and it has altogether so much of the same aspect, that I am tempted to believe that its constituents will be found the very same with those of the serpentine—an experiment which I shall certainly try, if my specimens reach me from Cornwall in safety.

Veins of schorl are said to occur in the clay slate near the Lizzard. Unless they constitute what is called by Mr. Jameson the schorl rock of Cornwall, I know not where to find it; as I neither saw nor heard of any rocks of schorl in the county (except in minute quantities), though I was on the look out for them during my whole stay in the county.

I think there is reason to doubt whether the greenstone and felspar porphyry reach to the west coast of the peninsula; for at Mullyan, which I conceive to be nearly opposite to the southern commencement of the greenstone on the east coast, I found a bed of clay slate. As it became dark soon after I passed Mullyan, I had no opportunity of seeing the structure of the country on my way north to Helston; but as the country round Helston itself is composed of clay slate, we have little reason to doubt that the same rock extends all the way south to Mullyan. The clay slate round Helston is of a dark blue colour, almost black, with hardly any lustre; nor could I detect any mica in it; but it is traversed in every direction by quartz veins. It has no resemblance to primitive clay slate; and though I found no greywacke, nor any other rock but the slate, I conceive, from its appearance and connection with the neighbouring country, there can be no hesitation in referring it to transition slate.

From Helston to Merazion is a distance of eight miles; and the whole country, as far as I had an opportunity of seeing it, is composed of the same transition slate. Merazion stands upon the sea shore upon the east shoulder of Mount's Bay. The rocks below it are exposed for about 740 yards along the coast, and in some places are about 20 feet above the level of the sea. Within that space I reckoned 14 alternate beds of clay slate, and a blue stone, to which Dr. Berger and Mr. Allan have given the name of greywacke. It is very hard; and the materials of which it is composed cannot be distinguished by the naked eye. Though no person could recognise it as greywacke merely from seeing specimens of it, yet, from its situation and connection, I cannot avoid thinking that these gentlemen have named it right. The clay slate is by far the predominant rock; but the beds of this greywacke rock are sometimes also pretty thick. The thickest which I observed was 73 yards, and the thinnest five feet thick.

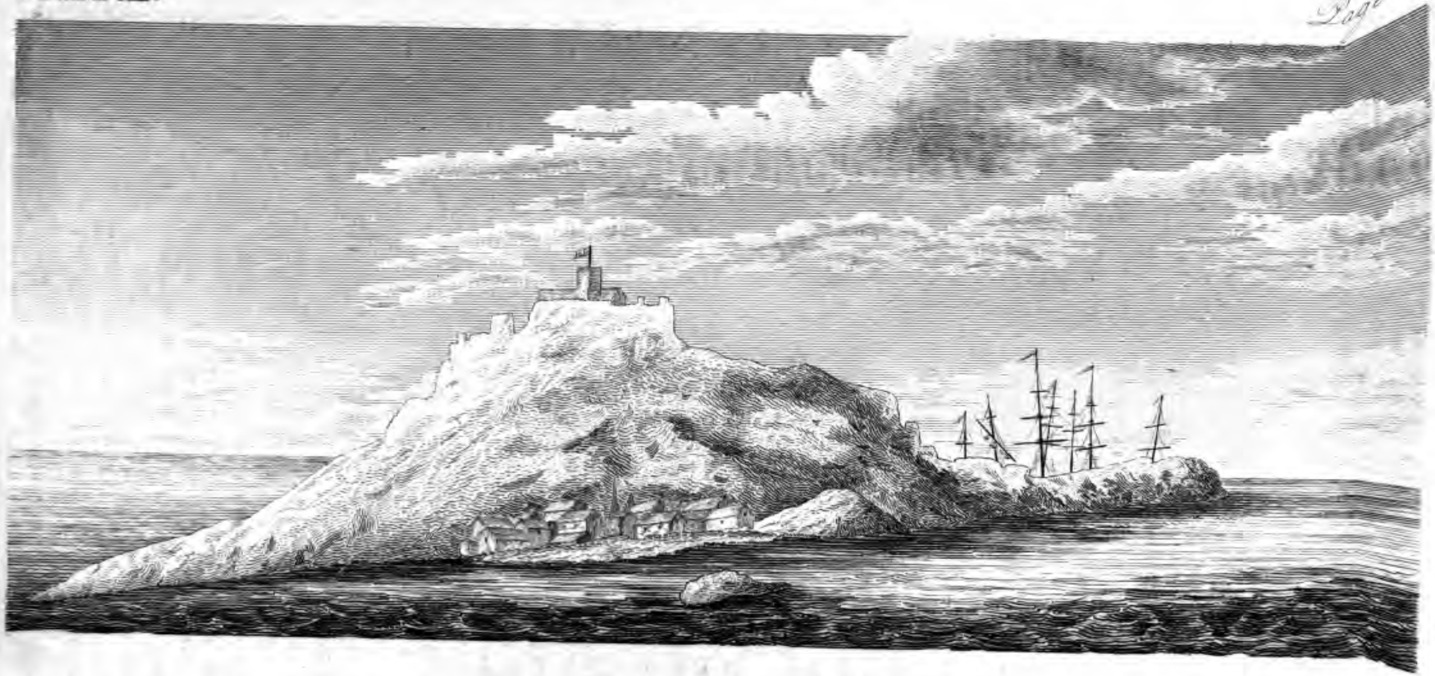
These clay slate and greywacke rocks lie opposite to St. Michael's Mount, which is about 500 yards south from the Dry Beach. They extend both farther east and farther west than the mount. Now it deserves notice that those beds which are on the east side of the middle of the mount dip east at an angle of about 70°, and those on the west side dip west at about the same angle. Hence if they were continued without interruption till they met, they would form a sloping hill considerably higher than the mount, and the mount itself would form the southern extremity of it. The farthest west bed of greywacke is the highest. Tin ore was formerly found in it, and a mine existed for some time from which it was extracted; but it has been for a considerable period abandoned.

The clay slate rocks cannot be traced without interruption from the sea shore to St. Michael's Mount, the middle part of the way for about 100 yards being covered by sea sand; but that they continue in reality under this sand cannot be doubted.

St. Michael's Mount itself is a small conical hill, according to Dr. Berger's measurement 231 feet high. As it is the most remarkable spot in Cornwall, in a mineralogical point of view, I have given an outline of its shape in the annexed plate. When we reach it from the north, the first rocks we meet with are clay slate, possessing exactly the same characters as the clay slate at Merazion, and undoubtedly a continuation of the same

PLATE XI.

Page 2



St. Michael's Mount from the North.

bed. The whole northern base of the mount consists of clay slate. I could not determine either the direction or amount of its dip; but it seemed to lie much more horizontally than the clay slate at Merazion. It does not extend to any height up the mount. The upper part of the mount consists of granite. On the south side this granite continues down to the water's edge, and it continues to constitute the whole of the hill both on the east and west side for about $\frac{3}{4}$ths of its whole extent. Then we come to the clay slate. On the east side, where the granite terminates, numerous veins of it run into the clay slate in different directions. I traced some of them for a length of 125 feet, and then lost them only because they ran under heaps of loose stones, which I was unable to remove. These veins vary in width from an inch or two to about a foot and a half; but their common width is above a foot. Sometimes they contain fragments of clay slate; sometimes they consist of felspar; but most commonly of perfect granite. I consider the descriptions which Mr. Playfair and Mr. Allan have given of them as correct.* No person who will be at the trouble to examine them can doubt that they are real veins, that they run from the granite into the clay slate, and therefore that the granite must have been deposited after the clay slate: but if there be any person not satisfied with the appearance of these veins, he has only to go to the west side of the mount, where he will find two beds of granite in the clay slate, and the position of these beds such as to indicate in the clearest manner that the great body of the granite had been deposited after the clay slate.

These facts put it beyond dispute that the granite of St. Michael's Mount is not *primitive* but *transition granite.* This is the conclusion that Mr. Allan ought to have drawn from his premises, and the conclusion that he would have drawn if he had given his reasoning powers fair play. He proved that the granite lay over the clay slate, and that it must have been deposited after the clay slate. He proved, likewise, that the clay slate is a transition rock. From all this it follows irresistibly that the granite likewise is a transition rock.

This is not the first time that granite has been observed as a transition rock. Von Buch observed it in a similar position in the neighbourhood of Christiana in Norway. In all probability, when transition rocks, which are so common in this country, come to be examined more carefully than they have hitherto been, granite will be frequently found among them. Why should it not, as well as clay slate, greenstone, and porphyry?

(*To be continued.*)

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* The clay slate in the neighbourhood of the veins contains so much mica as to have the aspect of mica slate.

Article III.

On the Nature of Muriatic Acid. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

(Letter from Dr. Berzelius to Dr. A. Marcet.*)

DEAR SIR,

You ask of me an explanation of my ideas respecting muriatic acid, and the reasons that prevent me from adopting the new theory of Sir Humphry Davy respecting this substance. I shall in the first place state the difference between the old theory and that of Sir H. Davy, and then give my reasons for considering the old theory as the most accurate.

According to the old hypothesis, muriatic acid is composed of a combustible radicle still unknown, and of oxygen. Muriatic, like several other acids, cannot be obtained in a separate state. It does not seem capable of existing except in combination with some oxide or other. When combined with water, it constitutes common muriatic acid gas. In this compound the water constitutes a base for the acid, just as I have proved it to do in concentrated sulphuric and nitric acids, in effloresced oxalic acid, &c. The muriatic radicle is capable of combining with different doses of oxygen. I have proved that muriatic acid neutralized by a base contains exactly twice as much oxygen as the base with which it is saturated; that is to say (to employ the expression of Dalton), that the acid is composed of one atom of radicle and two atoms of oxygen. The other compounds formed consist of one atom of radicle combined with three, four, and six atoms of oxygen, constituting oxymuriatic gas (superoxidum muriatosum), euchlorine gas of Davy (superoxidum muriaticum), and hyperoxymuriatic acid (acidum oxymuriaticum). Muriatic is one of the most powerful acids, and possesses in a high degree the property of forming with salifiable oxides, both neutral salts and salts with excess of base.

According to the new hypothesis, oxymuriatic gas, in the present state of our knowledge, must be considered as an elementary body, though its great specific gravity, and the property which it has of crystallizing with water at a low temperature, leads one to conjecture that it is a compound, and even that it contains oxygen. The illustrious author of this hypothesis has given it the name of *chlorine*. Chlorine has the property of combining with an atom of oxygen; and the oxide thus produced is called *euchlorine.* Chlorine combines likewise with an atom of hydrogen. The compound formed is muriatic acid. This acid is analagous to sulphureted hydrogen and telluretted hydrogen, and, like these two acids, it is decomposed by the greatest number of the saline bases, the oxygen of which combines with the hydrogen of the acid, and forms water, at the same time that the metallic radicle of the base unites with the chlorine. Muriatic acid parts with its hydrogen still more easily than the two above-mentioned acids, since it is decomposed by oxides which neither alter sulphureted nor telluretted hydrogen; by potash and soda, for example. On the other hand muriatic acid is neither decomposed by atmospheric air, nor oxygen gas, as happens to the compounds of hydrogen with sulphur and tellurium. What we have hitherto called muriate of potash and muriate of soda, is nothing but a compound of chlorine and the metallic radicles of these oxides. The only bases, which do not decompose muriatic acid, are ammonia, alumina, and in some measure also magnesia. The salts called *hyperoxymuriates* are combinations of euchlorine with the peroxides of the bases. Chlorine combines with all combustible bodies except *carbon,* and perhaps also *boron,* but it combines with the oxide of carbon.

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* Dr. Marcet, having been requested by Dr. Berzelius to publish this letter, in order to promote discussion, has complied with the request.

Such is an outline of each hypothesis. I shall in the first place examine the circumstances by which the illustrious author of the new hypothesis considered himself as obliged to abandon the old opinion. I shall afterwards state some facts which agree very well with the old hypothesis, but not with the new.

It has not escaped the sagacity of Davy that chlorine may contain oxygen; but having exposed salifiable bases to the action of oxymuriatic gas, he found that the quantity of oxygen evolved was exactly equal to that contained in the oxide employed. Hence he concluded that this oxygen could not be furnished by the oxymuriatic acid gas, but was derived from the oxide; the radicle of which, having a stronger affinity for chlorine than for oxygen, parted with its oxygen and united with the chlorine. Before the establishment of the doctrine of definite proportions in chemistry, the experiments just stated might certainly warrant a new conjecture respecting the nature of oxymuriatic gas; but at present it is established by that doctrine, that the oxymuriatic acid is composed of one atom of radicle and three atoms of oxygen; that it is capable of neutralizing a quantity of any base whatever, the oxygen in which amounts to half the oxygen in the acid; and that, of consequence, the quantity of oxygen disengaged from the oxymuriatic gas decomposed by a saline base is equal to that contained in the base by which it is decomposed.

Hydrogen gas unites with oxymuriatic gas, and forms a muriate of water, without any excess either of water or muriatic acid. This experiment, being conformable to the old hypothesis, proves nothing in favour of the new. Though the muriate of water has the gaseous form, this proves nothing against the old hypothesis; for the form of aggregation is a physical property which neither hypothesis is capable of explaining.

The strongest circumstance in favour of the new hypothesis is, that charcoal is neither capable of decomposing oxymuriatic gas nor muriate of silver at any temperature whatever. This circumstance, compared with the extreme facility with which oxymuriatic gas acts upon the metals, appears at first sight perfectly to justify the conjecture that oxymuriatic gas is an elementary substance; but the most simple explanation is not always the most accurate. In order to conceive the cause of this phenomenon we must attend to its connection with the general theory of chemistry. We must observe, in the first place, that there are several acids which cannot exist without being combined with water or with some other oxide; such are sulphuric, nitric, oxalic, tartaric acids, &c. Now we suppose that the same is the case with muriatic acid; and if so, a combustible body, the oxide of which formed by the quantity of oxygen contained in the oxymuriatic gas is insufficient to saturate all the quantity of muriatic acid contained in that gas, can never at any temperature whatever decompose oxymuriatic gas into oxygen and muriatic acid, because the muriatic acid has no base with which it can combine in order to preserve its existence as an acid. Such a combustible substance must either deprive the acid radicle of the whole of its oxygen, or, if its affinity for oxygen be not sufficiently great for that, it will leave the acid without any alteration at all; but we know that the oxide of carbon has not the property of combining with any other oxide. Hence charcoal cannot furnish an oxide capable of uniting with muriatic acid, when deprived of its excess of oxygen. If we suppose that the radicle of muriatic acid has a stronger affinity for oxygen than charcoal has, there is no difficulty in explaining the want of action of charcoal on oxymuriatic acid; while such a supposition throws considerable light on the theory of chemistry.

You see, then, that the facts which have been considered as most favourable to the new hypothesis are equally explicable on the old opinions.

If we attempt to prove by a direct experiment that the new theory is inconsistent with truth, it will be difficult to find any which is not susceptible of explanation, according to either supposition. The cause of this is, that as the muriatic acid cannot be obtained in a separate state, it always retains in combination enough water to oxidate the combustible bodies sufficiently to be capable of uniting with the pure acid. Hence we have it not in our power to make potassium act upon pure muriatic acid; because, whether we employ oxymuriatic gas or muriatic acid gas, there is always a sufficient quantity of oxygen present to convert the potassium into potash, without decomposing a single atom of muriatic acid.

But there are methods, less direct indeed, but not less sure, of coming to a decisive conclusion on this point. The doctrine of definite proportions affords us the means of calculating what cannot come directly under our hands; just as the astronomers have made the most striking discoveries by means of mathematical calculations. You entertain no doubts, I hope, respecting the truth of that doctrine. Now I shall show from it that there are combinations which, if explained according to the hypothesis of Davy, are inconsistent with well ascertained chemical proportions.

Such are all the submuriates which contain water of combination. I have proved by a set of experiments (which you will find in the *Annales de Chimie* for May and June 1812) that when a salt contains combined water, that water always exists in such a proportion that its oxygen is always either equal to that of the base, or it is a multiple or submultiple of it by a whole number. For example, the subsulphate of iron contains a quantity of water whose oxygen is equal to that of the oxygen in the oxide of iron, which constitutes the base of the salt. The subsulphate of copper is so constituted that the water, the acid, and the oxide, contain each the same quantity of oxygen. Hence we may conceive this salt to be composed of one atom of acid and three atoms of hydrate of copper. The new hypothesis admits the existence of a submuriate of copper. Let us examine the composition of this salt. We obtain the salt by pouring caustic alkali into the muriate of copper, taking care not to precipitate the whole of the oxide from the solution. The precipitate well washed and dried is the submuriate of copper. If we dissolve it in nitric acid, and precipitate the muriatic acid by nitrate of silver, we obtain a quantity of muriate of silver, indicating 12·1 parts of muriatic acid in 100 parts of the submuriate of copper. Digest the liquid over mercury to get rid of the superfluous silver; evaporate to dryness in a platinum crucible, and expose it to a red heat till all the acid and mercury are driven off: the oxide of copper obtained weighs 72 parts. Thus the acid and base in 100 parts of submuriate of copper amount to 84·1 parts. The 15·9 parts wanting must have been combined water. We may ascertain the existence of this water by heating the submuriate in a retort previously weighed, and furnished with a tube filled with muriate of lime. The subsalt, together with its water, loses a little oxygen gas which passes through the muriate of lime, leaving the water behind it. In this manner we obtain from 15·75 to 16 parts of water, according as the submuriate has been more or less perfectly dried. If we calculate this result, we find that 72 parts of oxide of copper contain 14·37 parts of oxygen, and that the quantity of oxygen in 15·9 parts of water is 14·32. Thus it appears that the oxygen in the water, as in the subsulphate, is equal to the oxygen in the oxide.* As we know that 100 parts of muriatic acid are neutralized by 147·27 parts of oxide of copper, that is to say, that the atom of acid is to the atom of oxide as 100 : 147·27, it follows that the subsalt is composed of one atom of acid and four atoms of hydrate of copper. Thus you see the admirable harmony that reigns in these compounds according to the old mode of explaining their composition.

Let us now consider the composition of this salt according to the new hypothesis. Muriatic acid, according to this hypothesis, is a compound of one atom of chlorine and one atom of hydrogen. Hence it follows that the submuriate contains 16·08 parts of muriatic acid for 72 parts of oxide of copper. The water in combination amounts only to 11·92 parts: but as analysis gives us 15·9, we must suppose the 3·98 of surplus formed by the decomposition of the acid; the hydrogen of which forms water with part of the oxygen of the oxide; at the same time that the metal of the oxide combines with the chlorine evolved. In the 11·92 parts of water admitted by the new hypothesis there are only 10·74 of oxygen, that is to say, $\frac{3}{4}$ths of the quantity contained in the oxide of copper. Hence the subsalt contains one atom of oxide and $\frac{3}{4}$ths of an atom of water; that is to say, that the water is to the base in a ratio inconsistent with the doctrine of definite proportions. Hence the explanation given by the new hypothesis cannot be accurate.

The submuriate of lead containing water (that is prepared by precipitation) furnishes still a better proof, because the water which it contains is more easily determined by direct experiment; but I chose the submuriate of copper in preference, because, if you think proper to examine what I have said, you may do it without any tedious experiment; you have only to compare what I have said respecting the submuriate of copper with the analysis of it published by Mr. John Davy in the Phil. Trans. for 1812.

If, on the other hand, we examine a submuriate without water of combination, as the submuriate of lead exposed to a red heat, we shall find that according to the old hypothesis the acid is united to one atom of oxide; while according to the new hypothesis these submuriates constitute a perfectly new class of bodies, composed of chlorine, metal, and a proportion of oxygen insufficient for the oxidation of that metal. The new hypothesis represents the composition of the submuriates destitute of water by one atom of chlorine, one atom of metal, and three atoms of oxide. I suppose the defenders of the new opinion will endeavour to reconcile it with the doctrine of definite proportions, by pretending that the submuriates with water are composed of one atom of cuprane (for example) and three atoms of hydrate of copper: but if such an allegation is made, we shall have a right to reproach them with seeking rather to defend an hypothesis at all hazards than to discover the truth.

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* If we calculate by the table given in the Annals of Philosophy, vol. ii. p. 32, we shall find that the oxygen in 72 parts of peroxide of copper amounts to 14·4; and the oxygen in 15·9 parts of water is 14·05. But these changes do not destroy the identity observed by Berzelius.—T.

The want of agreement between the new theory and the doctrine of definite proportions is such, that I am surprized it could ever gain credit. The muriate, nitrate, and sulphate of ammonia, possess analogous properties, and are all classed among ammoniacal salts. We find the same analogy among the compounds of potash with the same acids, and of course are disposed to class them all among the salts. But according to the new hypothesis, a muriate of potash cannot exist; and what was formerly considered as a salt contains, notwithstanding its exact analogy with the salts, neither an acid, nor oxygen, nor even potash itself. The new hypothesis admits a muriate of ammonia merely because we know no method of obtaining that salt free from water: but the same thing holds with the sulphate and nitrate of ammonia, and with almost all the ammoniacal salts; for boracic acid and arsenious acid, deprived of water, are not capable of absorbing the smallest portion of dry ammoniacal gas.

Let us attend to the muriate of iron formed by exposing iron to an excess of oxymuriatic acid. It is red, the colour of the oxide passing, as it were, through that of the acid with which it is in combination; yet according to the new hypothesis this body contains no oxygen. The compound formed by the action of light on a mixture of oxymuriatic acid and oxide of carbon, to which a name has been given of unlucky omen for the science, affords a fine illustration of the doctrine of chemical proportions, according to which bodies possessing the same electro-chemical modifications combine in such proportions that they contain equal quantities of oxygen. The old hypothesis considers it as composed of an atom of muriatic acid and an atom of carbonic acid, and consequently as analogous to the fluoboracic acid: while, according to the new hypothesis, it is composed of carbon and of two electro-negative bodies, chlorine and oxygen; that is, of one radicle and two (to use the expression) oxygens.

But I conceive what I have said to be sufficient to enable you to judge which of the two hypotheses explains the phenomena in the most plausible and satisfactory manner.

ARTICLE IV.

An Account of the Biddery Ware in India. By Benjamin Heyné, M.D. Naturalist to the Hon. East India Company at Madras.

THE Hindoos have since time immemorial not only excelled their neighbours in the management of metals for useful and curious purposes, but they are even familiarly acquainted with alloys unknown to our practical chemists.

Among those in general use that have drawn the attention of Europeans living in India, are the alloys for the gurry, and the Biddery ware.

The gurry is a disk of a cubit and upwards in diameter, about half an inch in thickness in the centre, but decreasing toward the circumference, where it is scarcely more than $\frac{1}{4}$ of an inch. It is used to mark the divisions of time, by striking it with a wooden mallet. The sound is in general remarkably clear, full, and loud, when it is properly managed. In common they are suspended on a triangular pyramid made of three bamboos tied together at top. They are used in all large cities, at the cutivals choultry, at the houses and cutcheries of great men, at the main guard of every battalion, and head-quarters of every detachment of troops. Some commanding officers have them even near their doors, to the annoyance of their visitors, whose ears are not so blunted and insensible as their own. In short, they are the regulators of time and business in all India. The exact proportion of the compound of which they are made I do not recollect, but I believe it is somewhat variable, as the gurries are prized according to the place where they have been manufactured.

The Biddery ware is used particularly for hooker-bottoms, and dishes to hand betel about to visitors, where more precious metals are not attainable. It is of a black colour, which never fades, and which, if tarnished, may be easily restored. To relieve the sable hue it is always more or less inlaid with silver. It is called Biddery ware from the place where it was originally, and I believe is still exclusively, made; for though the people of Bengal have utensils of this kind, I have nowhere seen any new ones for sale, which would be the case were they manufactured there.

Biddery is a large city, about 60 miles N.W. from Hyderabad, formerly the seat of mighty Kings, and one of the largest, or best places of the Dekan, belonging to the Nizam. It is situated on the eastern brink of a table-land, which is about

100 feet above the level of the surrounding country, and from S. to N. six to eight miles in its diameter. The place is fortified, has high walls and extensive outworks, particularly to the northward; but whether strong, or otherwise, I am not competent to judge. I found them very badly guarded; as is generally the case in the fortified places belonging to the native powers of India.

As I had been always very desirous of learning the composition of the Biddery ware, and could get no information of it at Hyderabad, I requested Captain Sydenham, then resident at that Court, to favour me with a dustuck (order) to the Governor of Biddery (which place I was to pass on my way to join the detachment at Joulna), to assist me in getting the desired knowledge. I must observe here, that it is not only extremely difficult, in general, for travellers, but almost impossible, without much money, to acquire any information on a subject of the most indifferent nature, without the concurrence and actual support of the head-man of the place. At Biddery the jealousy against Europeans of all classes is carried so far, that none are allowed to enter the gates of the city, except such as are in the service of the Nizam, and stationed in the fort. It happened fortunately that the chief of that place had some favours to ask of Captain Sydenham, and Mr. Russel, his assistant, whose kind assistance in promoting my inquiries on this and all other occasions I have gratefully to acknowledge: so that I received the dustuck without much delay, just as I ascended the table-land. On producing it at Biddery some of the manufacturers were immediately sent to me in the choultry, under a guard of peans, with the strictest orders that they should inform me of the whole and every part of their mystery. I wished to go to their houses; but as this had not been mentioned in the order, and as they lived in the city, I could not obtain permission. The men who attended me complained of want, in an employment which in former times had been the means of subsisting a numerous class of their own cast, and of enriching the place, but which now scarcely yielded food to five families that remained. They are of the goldsmith cast, which, together with some of other handicrafts, is the lowest of all sooders, though they wear the Bramінical string.

At their first visit they brought nothing but a lump of the compound used for casting their ware, and a few vessels which they had just in hand for inlaying them with silver, an operation which they conceived would be of all the most attractive to a curious faringa. As the metal in this state was divested of all but its natural colour, I recognized it immediately as a compound of which the greatest proportion is tin. It contained of this metal 24 parts, and 1 of copper, joined by fusion. I was herein not a little disappointed, as I had always understood that it was made of a metallic substance found on the table-land of Biddery, and which, as I never had made any experiment with a view of discovering its composition, I flattered myself might be a new mineral. In coming along I really had found also a lithamaga, which resembled the common Biddery ware in colour and appearance; and it was probably this that had given rise to the account which former travellers had given of that substance, as the mineral used for the ware manufactured at that place.

The business of their second visit was to cast, or to make, before me a vessel of their ware. The apparatus which they brought with them on the occasion consisted of a broken cutchery-pot, to serve as a furnace; a piece of bamboo about a foot long as a bellows, or blow-pipe; a form made of clay, exactly resembling a common hooker-bottom; and some wax, which probably had been used by several generations for the purpose for which it is yet employed.

The first operation was to cover the form with wax on all sides, which was done by winding a band, into which the wax was reduced as close as possible round it. A thin coat of clay was then laid over the wax, and, to fasten the outer to the inner clay form, some iron pins were driven through it in various directions. After this had been dried for some time in the sun, the wax was liquefied by putting the form in a place sufficiently heated, and discharged through the hole, by which the melted metal is poured in to occupy its place. It is scarcely necessary to say that when the metal is sufficiently cooled the form is broken, and the vessel found of the desired shape.

Colouring the ware with the standing black, for which they are celebrated, is the next, and in my opinion the most remarkable operation. It consists in taking equal parts of muriate of ammonia and saltpetre earth, such as is found at the bottom of old mud walls in old and populous villages in India, mixing them together with water, and rubbing the paste which is thus produced on the vessel, which has been previously scraped with a knife. The change of colour is almost instantaneous, and, what is surprising to me, lasting.

The saltpetre earth of this place has, when dry, a reddish colour, like the soil about Biddery. It is very likely that the carbonate, or oxide of iron, which it contains, is essentially necessary for the production of the black colour. The muriate and nitrate of lime, which is in considerable proportion in all earth from which saltpetre is manufactured in India, may be perhaps not an useless ingredient in this respect.

The hooker-bottoms of this ware happen sometimes to get tarnished, acquiring a brownish, or shillering colour, which is easily removed, and the black restored, by rubbing the whole surface with a little oil or butter.

As nothing looks handsome in the eyes of an Indian but what is glittering with gold or silver, it may be imagined that their hooker and betel dishes, which are chiefly used on festive occasions, are not left destitute of these ornaments; they are chiefly decorated with silver, in the form of festoons, fanciful flowers, and leaves. Sometimes I have seen a little gold interspersed.

The way of inlaying them is very simple; but of course as tedious as can well be imagined, and could be only practised where time is of little value. The parts of the projected figure are first cut out in silver leaf, which are placed in a piece of broken earthenware before the artist, who cuts with a pointed instrument the same figure on the vessel, applies the silver leaf, piece after piece, and gently hammers it into its place.

The greatest skill consists in tracing the pieces of the figure on the vessel exactly of the same size as they are in the silver leaf, and in this I have never seen they are mistaken.

They do their work very expeditiously, and will make any figure on copper with the greatest nicety, according to the sample which is laid before them.

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Note.—Mr. Wilkins informed Dr. Heyné that the Biddery ware is likewise manufactured in Benares, and he thinks that zinc is used as an alloy in that part of India. I examined a piece of a metal statue which Mr. Wilkins considered as Biddery ware: it was zinc alloyed with a very little copper.—T.

ARTICLE V.

Contributions towards a Chemical Knowledge of Manganese.

By Dr. John.

(*Continued from p.* 184.)

Action of Oxymuriatic Acid on Sulphate of Manganese.

When oxymuriatic acid gas (I allowed the gas, that it might be pure, to pass in the first place through a very weak alkaline solution,) is made to pass for some hours through a solution of this salt, it is partly precipitated, and the precipitate becomes still greater when the liquid is heated. A dark brown or black oxide separates, and the solution contains uncombined acid.

When the solution is so far evaporated that it congeals on cooling, the greatest part of the black oxide is again dissolved.

If the dry mass be dissolved in water, and filtered to separate a small portion of undissolved black oxide, the liquid, when properly concentrated, deposites reddish crystals consisting of sulphate and muriate of manganese. If the whole mass be digested in alcohol, and the filtered solution be evaporated, small crystals are obtained, which readily deliquesce in the open air, and which, when dissolved in water, yield a copious precipitate with nitrate of silver. Hence they obviously consist of muriate of manganese. The mother-ley from which these crystals have separated is very acid, but neither crystallizes nor yields any precipitate when treated with nitrate of silver; but muriate of barytes occasions a precipitate insoluble in acids. Hence the uncombined acid is obviously the sulphuric.

The theory of these appearances is obvious. The oxymuriatic acid gives out oxygen to the protoxide contained in the sulphate of manganese, which becomes in consequence insoluble, and falls down in the state of black oxide. This portion is again dissolved by the muriatic acid, and forms muriate of manganese, which is not decomposed by sulphuric acid.

Triple Compound of Manganese, Ammonia, and Sulphuric Acid.

If we mix a solution of sulphate of manganese (not fully saturated with the oxide) with ammonia to the point of complete saturation, and evaporate the filtered solution according to the method above described, we obtain rose-red, transparent, rhomboidal crystals, very much entangled in each other. They deliquesce in a moist atmosphere, and when treated with potash give out ammonia, but in other respects exhibit the properties of sulphate of manganese. Hence it follows that they consist of ammonia, sulphuric acid, and protoxide of manganese. I have not determined the proportion of the constituents of this salt.

c. *With black Oxide.*

The peroxide reduced to the state of an impalpable powder is likewise dissolved by sulphuric acid without the application of heat. The solution, in which there is always an excess of acid, has a very fine dark violet-blue colour. When it is diluted with water the colour becomes lighter, and passes gradually into crimson-red, and thence into blood-red. The solution is not altered by moderate evaporation. When a strong heat is applied the solution becomes colourless, the oxide loses a portion of its oxygen, and the whole assumes the properties of a solution of common sulphate of manganese with an excess of acid.

Tartaric acid likewise changes the peroxide into protoxide when it is mixed with the solution, and a moderate degree of heat is applied. The liquid becomes quite colourless without any precipitation taking place; and when an alkali is added, a precipitate falls in the state of a white powder.

The solution of black oxide is precipitated by the pure alkalies dark brown; by alkaline carbonates, dark reddish brown; and by the prussiates, yellowish brown. These precipitates become darker when exposed to the air. The precipitate thrown down by the alkaline carbonates contains no carbonic acid.

The solution of black oxide in sulphuric acid is incapable of crystallizing.

When the black oxide of manganese is digested for some hours in diluted sulphuric acid, an amethyst-red solution is obtained, in which there is an excess of acid. It cannot be made to yield regular crystals. Alkalies occasion a brownish red precipitate when mixed with this solution; a proof that the manganese contained in it is in a higher degree of oxidation than in the neutral solution.

Some very interesting experiments which Giobert made known in the year 1790 on this subject in the *Memoires des Sciences de Turin*, t. 5, *An.* 1790 *et* 1791, p. 23—35, and which he produced as a proof that sulphuric acid treated with black oxide of manganese attracted a portion of oxygen from the oxide, and by that means is converted into oxysulphuric acid, dissolving a portion of oxide, and by that means acquiring properties similar to oxymuriatic acid, as far as the destruction of colours is concerned—these experiments I have not repeated with the requisite care.

If we mix one part of pure black oxide with 1¼ parts of concentrated sulphuric acid in a coated retort connected with a pneumatic apparatus, it is well known that by the application of a heat, at first gentle, but gradually raised to redness, we obtain pure oxygen gas. In this case it is very evident that the affinity of the protoxide for the acid facilitates the evolution of the oxygen gas. When the black oxide is heated by itself we obtain also, as in the present case, only pure oxygen gas. In this case also the metal is reduced to the state of protoxide, in which state it is completely soluble in acids.

Water passes over into the receiver mixed with some sulphuric acid; and there remains in the retort, when the red heat has been continued for an hour, a white porous mass, which dissolves in water by the assistance of heat. This solution exhibits the same properties as that of metallic manganese in sulphuric acid. I have never been able to obtain (as some chemists assert) colourless crystals by evaporating this solution. Single crystals, indeed, may appear nearly colourless; but when they are in groups they appear evidently of a light rose-red colour. Perhaps the mass found in the retort, which has quite a white colour, has given origin to this assertion. This is a pretty good method of separating the iron usually combined with manganese in its natural state; for sulphate of iron, when exposed to a red heat, becomes insoluble in water.

Such is the action of sulphuric acid when the fire is raised only high enough to redden the retort; but quite other affinities take place when the heat is urged so as to melt the coated retort. In that case the salt is decomposed, the protoxide takes a portion of oxygen from the acid, and is changed into black oxide, while the acid make its escape in the state of sulphurous acid. I repeated this experiment twice, and each time obtained in the beak of the retort a portion of solid sulphuric acid, which dissolved in water with the evolution of the heat, and the hissing noise for which it is distinguished. We shall see hereafter that when oxymuriatic acid is passed through a solution of muriate of manganese it is very much disposed to crystallize. Can the metal give occasion to this crystallization? And supposing this conjecture to be verified, can we from thence draw any conclusions respecting the still enigmatical appearance of a fuming matter in oil of vitriol?

VI. *Action of Sulphurous Acid on Manganese.*

Hitherto I have only examined the action of sulphurous acid on carbonate of manganese. I made a current of pure sulphurous acid gas pass through a vessel containing distilled water, and thence into another vessel in which there was carbonate of manganese mixed with water. A strong effervescence took place, owing to the disengagement of carbonic acid. When the experiment was finished I separated the solution smelling strongly of sulphurous acid from a white granular powder lying in the bottom of the vessel, which, as I shall show presently, was sulphite of manganese.

The filtrated solution being evaporated, the sulphurous acid was gradually converted into sulphuric acid, and the solution exhibited the properties of sulphate of manganese with an excess of acid.

Properties of Sulphite of Manganese.

It appeared in the state of a white, granular powder, destitute of taste, and insoluble in water and alcohol.

It is not altered by exposure to the air, and therefore may be examined without difficulty. I left a portion of this salt for three weeks in a small open vessel in a room: but it did not undergo the smallest alteration.

Sulphuric, muriatic, and nitric acids, drive off the sulphurous acid when the salt is exposed to their action, and we obtain sulphate, muriate, or nitrate of manganese.

When heated in a crucible, the acid is disengaged, and a brown oxide remains, which may be treated with acids without emitting any smell of sulphurous acid.

To determine in some measure the proportion of the constituents of this salt I treated 50 grains of it with very diluted sulphuric acid, and when all the sulphurous acid was separated, mixed the liquid with more water, and precipitated the manganese by carbonate of potash. The carbonate of manganese obtained weighed, after being washed and dried, 36 grains, which is equivalent to $20\frac{1}{10}$ grains of protoxide. Hence 100 parts of sulphite of manganese are composed of

Protoxide	40·2
Acid Water	59·8
	100·0

VII. *Action of Muriatic Acid on Manganese.*

(A.) *On the Metal.*

The metal is readily dissolved by muriatic acid of every degree of concentration with the evolution of hydrogen gas, and, when the acid is not too much diluted, with the production of considerable heat.

The solution when concentrated has a colour approaching rose-red, and deposites crystals of a similar colour.

(B.) *On the Protoxide.*

The pure protoxide dissolves in this acid quietly, and the carbonate with a strong effervescence. The solution has a rose-red colour.

Chemists have hitherto affirmed that this salt cannot be procured in the state of regular crystals. I have, however, been able to procure it in as regular crystals as any salt whatever. My method was this: I procured a quantity of concrete salt by evaporating to dryness a solution of manganese in muriatic acid. This I put into a solution, quite neutral, and already evaporated in a cylindrical porcelain vessel till a cuticle had formed on its surface. The vessel was placed for some days, as I have described in a preceding paragraph, in a temperature between 77° and 88°.

Properties of crystallized Muriate of Manganese.

It crystallizes in very fine, longish, thick four-sided tables, both extremities of which are pointed with two plains, and the edges are frequently truncated. Sometimes the truncature is so great as to change the crystals into six-sided tables. They are set upon one another.

They have a rose-red colour, are perfectly transparent, and have a caustic taste, which leaves a saltish impression upon the tongue.

They speedily deliquesce in the air. They may be very well preserved in a close vessel at the temperature of 77°; but when the temperature is a little higher they lose their transparency, and become covered over with a white powder, owing to the loss of a part of their water of crystallization; yet they do not fall completely to powder. When strongly heated in a retort they melt immediately, and lose their water of crystallization; and in a red heat they are in a great measure decomposed, the acid passes over into the receiver, and there remains in the retort a black shining crystalline mass, which still contains some acid.

Not only water, but alcohol also dissolves more than its own weight of this salt. The alcoholic solution burns, when in contact with a combustible substance, with a lively red flame, and emits frequent sparks. When the alcoholic solution is slowly evaporated the salt crystallizes in very fine, thin, transparent tables, and sometimes, when the evaporation is rapid, in very fine flat needles.

The specific gravity of the crystals is 1·560, that of water being 1·000.

In other respects the solution of these crystals exhibits the properties of the solution of sulphate of manganese.

Sulphuric acid does not decompose the solution of muriate of manganese.

Estimate of the Proportions of the Constituents.

A hundred grains of dry crystallized muriate of manganese were dissolved in water, and mixed with a solution of nitrate of silver as long as any precipitate fell. This precipitate being separated by the filter, washed and dried, and heated to the point of fusion in a silver vessel, weighed 130 grains. Now it follows from Klaproth's experiments that 133 grains of such a horn silver contain 20½ grains of muriatic acid. Hence the proportion of acid in 130 grains may be estimated at 20$\frac{1}{20}$ grains.

After the separation of the excess of silver I precipitated the whole of the manganese by means of carbonate of ammonia, warmed the solution a little, and carefully dried the precipitate. It weighed 69 grains, which is equivalent to 38½ grains of protoxide of manganese.

As the loss in the 100 grains of muriate of manganese examined must be ascribed to water, it follows that the salt is composed of

Protoxide	38·50
Acid	20·04
Water	41·46
	100·00

Remark.—This analysis corresponds very nearly with that of Bucholz given in the second volume of his Beitrage: but the method which he followed can only be practised by a chemist whose eyes are very much exercised.

Action of Oxymuriatic Acid on Muriate of Manganese.

I dissolved 300 grains of muriate of manganese in about 12 ounces of water, and made a current of oxymuriatic acid pass through the solution for six hours without interruption. The liquid began soon to froth, and the foam collected upon the surface; it acquired a yellow colour, and concreted into a crystalline mass. This change of state made it difficult for the gas to continue to pass. I allowed the apparatus to remain for a day without touching it, and found next day that a part of the mass had again become liquid, but that a great many long needle-form crystals had shot in it. When the glass was opened the smell of oxymuriatic acid gas was so strong as to be scarcely supportable. I threw the whole upon the filter, in order to separate the crystals from the liquid; but the liquid passed through, and the crystals speedily deliquesced and followed it.* It will appear from what follows that these crystals consisted of oxymuriatic acid and muriate of manganese. Here we observe the crystallization of the acid in combination with a metal. Nor have I found the same thing to take place when oxymuriatic acid is passed through an alkaline solution; and I do not believe that any person has observed oxymuriatic acid to crystallize at the temperature of 41°.

This solution is precipitated, by alkaline carbonates, brownish red; by the pure alkalies, brown; and by the alkaline prussiates, yellowish brown. The precipitate soon becomes darker by exposure to the air. The same thing happens to the above-mentioned crystals when dissolved in water. Whether in this compound the manganese be really in the state of brown oxide, in which state the precipitate appears, or whether it does not receive oxygen during its precipitation from the uncombined oxymuriatic acid, obviously present in the liquid from the smell, are questions which I cannot at present answer. However, the last opinion is very probable.

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* The temperature during the experiment was 41°, the filtration was performed at the temperature of 55°.—J.

When the solution is evaporated the smell of oxymuriatic acid gradually disappears, and at the same time black oxide is deposited. The solution assumes a red colour, contains uncombined muriatic acid, as is shown by re-agents, is precipitated white by the alkaline carbonates, and exhibits the properties of a common solution of muriate of manganese with excess of acid. The oxymuriatic acid is decomposed, it gives out a portion of oxygen to the oxide, and is changed into common muriatic acid. By farther evaporation the evolution of oxymuriatic acid again commences, the precipitated oxide is again dissolved, and we obtain finally the very same quantity of muriate of manganese at first employed.

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To these experiments I will add a few words respecting a new metal which I have found in the Saxon grey ore of manganese, the nature of which I hope soon to make evident.

This substance is distinguished from all bodies hitherto known by the following properties:

It appears to occur in the ore in the state of an acid, and it may be separated and volatilized by the action of other mineral acids upon the ore. If the operation be performed in a distilling vessel, and an opportunity be given to the disengaged acid to combine with potash or soda ley, a combination takes place, and a crimson-red solution is obtained, which is not altered by exposure to the air, being in this respect similar to some of the alkaline solutions of manganese. If this solution be mixed with another acid, and heat applied in a retort, the metallic acid is volatilized with a peculiar smell, and the residue consists of a combination of the acid employed with the alkali, without the smallest indication of any other substance, as far as I have been able to discover.

The crimson-red solution (which I consider as a combination of a peculiar metallic acid with an alkali) is precipitated by gallic acid and by tincture of nutgalls chesnut brown.

Alkaline prussiates produce no precipitate, but the red colour of the solution is immediately changed to a fine lemon yellow. The solution is not altered by the alkaline carbonates.

When mixed with some alcohol, and heated a little, it assumes a green colour, gives out an ethereal smell, and alkalies precipitate from it a brown oxide, which is soluble in muriatic acid.

When I treated pure oxide of manganese in the same way I obtained none of these appearances.

This substance seems to exist only in a small proportion in the grey ore of manganese. The experiments must be made upon pounds of the ore at once.

Remarks on the preceding Paper. By the Editor.

I considered the preceding paper as highly worthy of publication, because it contains many circumstances respecting manganese with which chemists ought to be acquainted, but which do not seem to be generally known in this country. At the same time the paper exhibits some striking imperfections, which it may be proper to notice for the sake of our chemical readers who may not have an opportunity of knowing exactly the present state of this part of chemical science.

1. Dr. John's method of procuring pure oxide of manganese is exceptionable. How he was able to succeed by means of it we cannot say; but certainly no other person has been able to verify his results. We shall insert a paper on the subject by Bucholz, which will convince our readers of the truth of our remarks. With respect to the separation of iron and manganese there are four methods of proceeding, all of which succeed tolerably well. 1. Gehlen's method is the best. It consists in dropping succinate of potash or succinate of ammonia into the impure manganese solution, previously brought as near as possible to a state of neutralization. The whole iron falls in the state of succinate, while the manganese remains in solution. The objection to this method is the high price of succinic acid, which Gehlen endeavours to obviate by saying that the succinic acid may be recovered by boiling the precipitated succinate of iron in an alkaline ley; but every practical chemist must be aware that such a proposal is quite absurd. The time lost, and the great trouble, would be more than equivalent to the price of the succinic acid. 2. Berzelius, in order to obviate this objection, substituted benzoic acid for succinic. It appears from the experiments of Hisinger (*Afhandlingar i fysik, Kemi och Mineralogi*, t. 3, p. 152,) that this method answers pretty well. 3. The impure oxide of manganese may be dissolved in sulphuric acid, the solution evaporated to dryness, and exposed to a red heat. The sulphate of iron is decomposed by this process; but the sulphate of manganese remains unaltered. Hence the latter continues soluble in water; but not the former. This process was proposed by Dr. John in the preceding paper. 4. The impure manganese may be dissolved in muriatic acid, the solution evaporated to dryness, and exposed to a red heat in close vessels. The ferrane sublimes, while the manganesane remains unaltered. This process may be repeated two or three times, and then the oxide of manganese will be freed from iron. This process was

proposed by Mr. John Davy. I do not know whether it has ever been put in practice.

2. The preceding paper contains only a part of Dr. John's experiments on manganese. We shall publish the sequel of his experiments in our next Number. The results which he obtained in his endeavours to determine the number and composition of the oxides of manganese will be there seen. He obtained three oxides, the colours and composition of which were as follows:

1. Green, composed of 100 metal + 15 oxygen.
2. Brown, ...100 ..... + 25
3. Black,............100 ..... + 40

Berzelius has given us the result of his experiments on the oxides of manganese (*Lärbok i Kemien*, t. ii. p. 129). According to him there are five oxides of manganese. He obtained the first by keeping manganese for two years in a very well corked glass. It fell down into a brownish grey powder, which filled the whole of the phial. It was semicrystalline, and had the metalline lustre. He found it composed of 100 metal and 7·0266 oxygen. He deduces the composition of the other oxides of manganese, partly from his own experiments, and partly from those of other chemists, as follows:—

	Metal.		Metal.	Oxygen.
1. Composed of	100	+ 7·0266 oxygen, or	1	+ 1
2.	100	+ 14·0533	1	+ 2
3.	100	+ 28·1070	1	+ 4
4.	100	+ 42·16	1	+ 6
5.	100	+ 56·213	1	+ 8

Sir Humphry Davy has given us the result of his experiments on the oxides of manganese. He obtained two oxides; the first dark olive, the second brownish black. He found their composition as follows (*Elements of Chemical Philosophy*, vol. i. p. 367):—

	Metal.	
1. Olive	100	+ 26·58 oxygen
2. Brown	100	+ 39·82

It is hardly necessary to mention the experiments of Bergman, as they were made at a time when accuracy could scarcely be expected. He obtained three oxides, which he found composed as follows:—

	Metal.	
1. Composed of	100	+ 25 oxygen
2.	100	+ 35
3.	100	+ 66·6

By arranging the proportion of oxygen obtained by the different experimenters, as in the following table, we may readily compare them together:—

	Oxygen by			
Oxides.	John.	Berzelius.	Davy.	Bergman.
1.	—	7·0266	—	—
2.	15	14·0533	—	—
3.	25	28·107	26·58	25
4.	40	42·16	39·82	35
5.	—	56·213	—	66·6

All the experimenters agree very nearly in the composition of the third oxide. Nor is the difference very great in the composition of the fourth. It is clear, from the experiments of John, that it is with the green oxide, or second oxide of Berzelius, that all the acids combine. Hence this is the oxide which constitutes the base of all the salts.

The first or protoxide of every metal must be a compound of one atom of metal with one atom of oxygen. Hence its analysis gives us the relative weights of an atom of the metal and an atom of oxygen. If we consider Berzelius's first oxide as the protoxide of manganese, it follows that the weight of an atom of manganese is to the weight of an atom of oxygen as 100 : 7·0266. I have in my table, published in vol. ii. p. 42, of this journal, adopted 1 to represent the weight of an atom of oxygen. And an atom of manganese, on the supposition that Berzelius's first oxide is accurate, would be 14·260. The oxides of manganese given by Berzelius will be composed as follows:—

1st Oxide of 1 atom manganese + 1 atom oxygen
2d Oxide of 1 .... + 2
3d Oxide of 1 .... + 4
4th Oxide of 1 .... + 6
5th Oxide of 1 .... + 8

But if we suppose that the first oxide of Berzelius does not exist (and his mode of obtaining it was far from satisfactory), in that case the green oxide of John will be the real protoxide, and the constitution of the respective oxides of manganese will be as follows:—

1. Protoxide, composed 1 atom metal + 1 atom oxygen.
2. Deutoxide, ... 1 .... + 2
3. Tritoxide, ... 1 .... + 3
4. Peroxide, ... 1 .... + 4

I confess this appears to me by far the most probable statement. In that case, the weight of an atom of manganese will be only 7·130: and the weight of an integrant particle of each of these oxides will be as follows:—

Protoxide	8·130
Deutoxide	9·130
Tritoxide	10·130
Peroxide	11·130

3. The only salts of manganese of which the composition is given in the preceding paper are the carbonate, sulphate, sulphite, and muriate. These, according to the analysis of John, are composed as follows:—

1. Carbonate of 100 protoxide + 61·17 acid + 17·91 water.
2. Sulphate....100........+ 108·53....+ 114·
3. Sulphite ....100........+ 94·45....+ 54·43
4. Muriate ....100........+ 52·05....+ 107·68

Or perhaps the composition will be more evident by stating it in the following manner, taking the weight of an atom of protoxide as the basis:—

	Protoxide.		Acid.		Water.
Carbonate	8·130	+	4·972	+	1·455
Sulphate	8·130	+	8·823	+	9·268
Sulphite	8·130	+	7·678	+	4·425
Muriate	8·130	+	4·431	+	8·754

There can be little doubt that the weight of an integrant particle of each of the acids which enters into the composition of these salts is as follows:—

Carbonic acid	2·751
Sulphuric acid	5·000
Sulphurous acid	4·000
Muriatic acid	2·365

An integrant particle of water weighs 1·132. If we compare these numbers with the preceding table we shall find an approximation to two integrant particles of each acid combined with one integrant particle of protoxide in each of the salts. The reason why the numbers do not accord exactly with two integrant particles of the acid is doubtless because some error has been committed in determining the composition, either of the protoxide of manganese, or of the salts into which it enters. Both determinations are probably erroneous to a certain amount. If the quantity of oxygen in the protoxide were a very little less than Berzelius found it, the weight of an integrant particle of the protoxide would be somewhat greater, and this would augment the number indicating the proportion of acid somewhat.

With respect to the water contained in these salts, it would appear that the carbonate contains 1 atom, the sulphate 9, the sulphite 4, and the muriate 8 atoms.

It was first observed by Richter that when one metal is precipitated from a solution by another the solution continues as neutral as at first. In this case the metal is changed, but the acid and the oxygen remains unchanged. For example, if we have a solution of sulphate of copper, a plate of zinc introduced throws down the copper. The copper in the solution was united with a certain proportion of oxygen and sulphuric acid; the zinc which has taken its place is now dissolved with the very same oxygen and sulphuric acid; so that the sulphuric acid now neutralizes a quantity of zinc containing just the same quantity of oxygen that the copper did which it formerly neutralized. From this it follows that acids (as far at least as those metals are concerned which throw down each other) always neutralize portions of oxides containing the very same numerical quantity of oxygen.

It has been ascertained that the quantity of oxygen in the oxides neutralized by 100 parts of the preceding acids is as follows:—

Carbonic acid	36·68
Sulphuric acid	20·02
Sulphurous acid	20·02
Muriatic acid	30·49

Now the quantities of protoxide of manganese, which contain these respective numbers of oxygen, are the following:—

36·68	oxygen are contained in	297·68 protoxide
20·02		162·47
20·02		162·47
30·49		247·45

If we compare these numbers with the composition of the salts as contained in Dr. John's paper, we shall find that the rule by no means holds with respect to the salts of manganese. According to his analysis,

100	carbonic acid saturate	163·46 protoxide
100	sulphuric acid	92·10
100	sulphurous acid	105·87
100	muriatic acid	192·11

It would appear from this statement that the law ought not to be extended to those metals that are not precipitated by others in the metallic state. Probably the nonexistence of this law with respect to them is the reason why they are not precipitated.

4. The supposed new metal announced by John in the pre-

ceding paper as existing in the Saxon grey ore of manganese was examined, soon after the publication of the paper, by Berzelius; and the appearances were shown by him not to depend upon the presence of any new metal, but to be owing to manganese.

Article VI.

Experiments on the Nature of Azote, of Hydrogen, and of Ammonia, and upon the Degrees of Oxidation of which Azote is susceptible. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry and Pharmacy in Stockholm.*

The following experiments are to be considered as a continuation of those which I have already published on the same subject in the *Annales de Chimie* of Paris, and in the *Annalen der Physique* of Gilbert, during the years 1811 and 1812. I have endeavoured to prove that azote contains oxygen, and that nitric acid neutralized by a saline base contains six times as much oxygen as that base. By the most accurate of my experiments I have found that the nitrate of lead is composed of

Oxide of lead	67·3
Nitric acid	32·7
	100·0

Hence 100 parts of nitric acid are neutralized by 205·31 of oxide of lead, a quantity which contains 14·715 of oxygen. In the nitrate of barytes 100 parts of acid are combined with 140·73 parts of barytes, which contain 14·73 of oxygen (*Ann. de Chim. Nov.* 1811, p. 174). Thus these two analyses mutually confirm each other.

I have endeavoured to prove that it is a general law that when two oxidized substances combine, the oxygen contained in the one is always either equal in quantity to that contained in the other, or it is a multiple of it by a whole number. If we admit this law to be correct, it follows from it that 100 parts of nitric acid ought to contain a certain number of times 14·715 of oxygen. This number of times must be greater than 4, because decisive experiments have demonstrated that the acid contains more oxygen than 14·715 × 4 : but it cannot be greater than 7, because that constitutes more than the weight of the acid. Hence the acid must contain either 5 or 6 times 14·715 of oxygen. But nitric acid has the property of forming with oxide of lead several salts with excess of base; as I have shown in my memoir on the composition of salts with excess of base (*Ann. de Chimie, June* 1812). And among these subnitrates there is one in which the acid is combined with twice as much base as in the neutral nitrate: but if the above-mentioned law be just, it is clear that the oxygen in nitric acid cannot be 5 times 14·715; because, were that the case, the oxygen of the acid in the subnitrate just mentioned would not be a multiple of that in the base by a whole number, but by $2\frac{1}{2}$; consequently the true quantity of oxygen in 100 parts of nitric acid must be 6 times 14·715, or 88·29. But as the most exact experimenters have not indicated so great a quantity of oxygen in nitric acid, it is necessary that the oxygen wanting should be contained in the *azote*, which of course must be a compound of oxygen and a combustible radicle.

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* It may be proper to mention, for the information of Dr. Berzelius, that this paper, though dated the 22d April, did not reach the Editor till the end of August.

Such are the conclusions which I have drawn from my experiments hitherto published. I do not know how far chemical philosophers will allow them to be well founded; but, in hopes that the laws of chemical proportions which I have endeavoured to establish will be one day examined and admitted, I will continue in this paper my researches respecting the nature of the bodies announced in the title; and I shall commence with the composition of nitric acid, supposing it to have azote for its radicle.

It is well known that M. Gay-Lussac accompanied his important discovery of the proportions in which gaseous bodies combine by an analysis of the oxides of azote, and of nitrous and nitric acids. He found that nitric acid contains one volume of azotic gas and two volumes of oxygen gas, which amounts in weight to 30·5 azote, and 69·5 oxygen. Messrs. Cavendish and Davy had obtained nearly the same result from their analytical experiments on this acid. These excellent authorities induced me to confide in this determination; and in my analysis of the subnitrates and subnitrites I obtained a satisfactory confirmation of it; that is to say, I thought I obtained a subnitrate such that the acid and oxide contained each an equal quantity of oxygen, supposing the acid composed as above stated. I was struck with this anomaly; and I have stated (*Ann. de Chim. June* 1812) the experiments which it induced me to make, though they led me to no satisfactory explanation. In consequence of the new determination of the composition of nitric acid by Dalton and Davy, I have been engaged to resume my researches, and have been happy enough to see the anomaly disappear, and to obtain results which throw much light on the nature of azote.

Mr. Dalton, who has given, in his *New System of Chemical Philosophy*, a corpuscular theory from which the doctrine of definite proportions follows as a necessary consequence, has stated (vol. ii p. 329), that the smallest quantity of nitrous gas, which in his experiments was condensed by 100 parts in volume of oxygen gas, was always 130. If we convert these volumes into weights we obtain nearly 73·5 oxygen and 26·5 azote. When I compared this determination with the results of my analyses of the nitrates, I found, to my great surprise, that they accorded much better with it than with the determination of Gay-Lussac, though the difference in the weights that ought to be obtained in the analysis of the nitrates, nitrate of ammonia for example, according to the one or the other of these determinations, was so little, that it was impossible to decide by experiment which of the two was the most just.

During my residence in London last summer, Sir Humphry Davy informed me that he had found, by experiments made with the greatest care, that nitric acid is composed in volume of 100 parts of azote and 250 of oxygen; that is to say, of 26·5 parts, by weight, of azote, and 73·5 of oxygen: for when nitrate of ammonia is distilled in a moderate heat, it is completely changed into nitrous oxide gas and water, without any trace of azotic gas. In the *Elements of Chemistry* published soon after by this illustrious chemist he has represented nitric acid as a compound of one proportion of azote and five of oxygen. It follows that in the neutral nitrates the acid saturates a quantity of base of which the oxygen is $\frac{1}{5}$th of that of the acid, supposing azote to be the radicle of this acid. As Davy has not admitted the result of the calculations which I have made on the composition of azote, he considers this last substance as an element, and of course he makes no inquiry about the sixth portion of oxygen which ought to exist in the azote. The only experiments of mine which did not agree with the determination of this celebrated chemist were the analyses of the subnitrates and subnitrites of lead at a maximum. I determined, therefore, to repeat them; and I shall now state the result which I obtained.

The analysis of the subnitrate of lead at a maximum (mentioned in my treatise on the composition of the subsalts) being very simple, and very easy to be made with precision, I had no reason to doubt its exactness. It was more probable that I had committed some error in the method of procuring the salt. I might have obtained a mixture of hydrate of lead and subnitrate of lead, or a mixture of two different subnitrates of lead. I resolved, therefore, to examine the precipitates produced in a solution of nitrate of lead by different quantities of ammonia.

Subnitrate of Lead at a Maximum.—The precipitate obtained by pouring an excess of ammonia into a solution of nitrate of lead was digested for 24 hours in concentrated and caustic ammonia. It was then washed upon a filter till every trace of ammonia disappeared. When dried at a temperature a little above 212°, it assumed the form of a white powder. When applied to the tongue, it had a weak, but very styptic taste. It dissolved readily in nitric acid without effervescence, and of course contained no carbonic acid. Neither did it contain any ammonia, as caustic potash decomposed it without the evolution of any ammoniacal odour.

Ten parts (amounting to 154·44 grain troy) of this salt exposed in a small crucible, exactly weighed, and placed in a sand-bath to a heat considerably higher than that of boiling water, gave out pure water, and assumed a yellow colour. The loss of weight was 0·18. The heat being raised still higher, a little water was disengaged, but it was accompanied by red fumes. The loss now amounted to 0·198. As the last portions of water were disengaged at the same time with the acid, I cannot determine the quantity of water with rigorous exactness: but this experiment shows us that the proportion of water exceeds a little 1·8 per cent., but does not amount to 1·98. The residue, heated in the crucible till the acid was completely expelled, left a quantity of yellow oxide of lead weighing 9·08.

To ascertain whether this subnitrate could be always produced with the same properties and composition when the excess of ammonia employed was great, I prepared a new quantity of it, and obtained exactly the same results; 10 parts of it left 9·064 of yellow oxide, and furnished 0·185 of water, containing traces of acid. The subnitrate at a maximum, then, is composed as follows:—

Oxide of lead	90·80
Nitric acid	7·37
Water of combination	1·83
	100·[*]

But 90·8 parts of yellow oxide of lead contain 6·492 of oxygen. The 7·37 of nitric acid, if we consider it as composed of oxygen and azote, contain only 5·417; but such a composition is inconsistent with the laws of chemical proportions. On the other hand, if we suppose azote to be an oxide, and nitric acid to be composed as we have stated at the beginning of this dissertation, then the 7·37 of acid will contain 6·508 of oxygen, that is to say, the same quantity with the oxide of lead. The 1·83 of water contains 1·615 of oxygen, which multiplied by four gives 6·46. Hence it follows that the oxygen of the water is $\frac{1}{4}$th of that of the base; and if the water be represented by 1, the acid and oxide will be represented each by 4. On the other hand, 73·7 of acid are combined with 908 of oxide; so that 100 parts of acid combine with 1236 of oxide. We have seen that in the neutral nitrate 100 parts of acid are combined with 205·81 parts of oxide. Now 205·81 × 6 = 1234·86; that is to say, that in the subnitrate at a maximum the nitric acid is combined with six times the quantity of base with which it is combined in the neutral nitrate.

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[*] The mean of two experiments.

Intermediate Subnitrate of Lead.—I precipitated a portion of nitrate of lead by ammonia in excess, but without digesting it in pure ammonia. The precipitate was white; and when the liquid was passed through the filter I perceived that the salt began to dissolve in proportion as the ammoniacal liquor was removed; but it precipitated again in proportion as the drops fell into the liquid that had already passed through the filter. The precipitate, when washed and dried, formed a white powder; 10 parts of which exposed in a crucible to a sand heat lost 0·292 of water, and became pale yellow. Being then heated till the whole of the acid was removed, there remained 8·584 of yellow oxide. As such a composition does not accord with the laws of chemical proportions, we must conclude that this precipitate is a mixture of subnitrate at a maximum, and of that in which the acid is combined with twice the quantity of base that exists in the neutral nitrate.

To obtain the intermediate subnitrate I endeavoured to employ the smallest possible excess of ammonia. I precipitated, by ammonia in a very small excess, a solution of nitrate of lead, and I digested the solution in the excess employed. To decompose the subsalt at a maximum which might have been formed, I poured drop by drop a weak solution of nitrate of lead into the mixture, and digested it for half an hour. I then added a little more of the nitrate, and digested again; and I continued this process till the liquor contained no longer any excess of ammonia, excepting a portion so small that it could no longer be recognized by the smell, but became sensible by means of a stopper dipped in muriatic acid. I then filtered, and the precipitate exhibited the same phenomenon of partially dissolving again, already mentioned. When about the fourth part of this precipitate was carried away by edulcoration I considered the residue as sufficiently edulcorated, and therefore dried it.

The subsalt thus obtained was analysed in the same manner as the preceding specimens. It yielded pure water, and assumed, while still hot, a yellowish colour, but which on cooling was changed into white. Ten parts of it produced 0·35 parts of water, and left, when the acid was driven off, 8·298 parts of yellow oxide. Hence the portion of acid amounts to 1·352. Hence this subsalt is composed of

Oxide of lead	82·98
Nitric acid	13·52
Water of combination	3·50
	100·00

Now 82·98 of oxide contain 5·933 of oxygen, 13·52 of acid contain 11·93, and 3·5 of water contain 3·08: but $\frac{11\cdot93}{2}$ = 5·965, and 3·08 × 2 = 6·16. Hence in this salt the acid contains twice as much oxygen as the oxide, and the oxide twice as much as the water. Farther, we find that 135·2 of acid are united with 829·8 of oxide, therefore 100 of acid combine with 615 of oxide; but 205·81 × 3 = 617·43; that is to say, that in this salt the acid is combined with three times as much oxide as in the neutral nitrate.

I ought to observe that I repeated a second time the preparation and analysis of this subsalt, and obtained as the result 82·975 oxide, 3·25 of water, and 13·775 of acid. The slight difference between these two experiments is to be ascribed to the great care and patience necessary to procure the subsalt entirely free from all admixture of the two other subsalts; for when there is a slight excess of ammonia a small quantity of the subnitrate at a maximum is formed: and when too little ammonia has been employed, or the mixture not digested sufficiently long, the precipitate is contaminated with the subsalt at a minimum: and in this last case the quantity of water diminishes in proportion as that of the acid augments.

These experiments prove that the subnitrate at a maximum, of which I have given a description and analysis in my essay already quoted, was in reality merely a mixture of the two subnitrates just examined: and consequently that a subnitrate in which the acid saturates $1\frac{3}{4}$ as much of base as in the neutral nitrate does not exist.

Nitric acid, then, has the property of combining with four different proportions of oxide of lead; that is to say, *a*, with 205·81 of oxide, forming the neutral nitrate; *b*, with twice that quantity = 411·62, forming the subnitrate at a minimum; *c*, with 205·81 × 3 = 617·43, forming the intermediate subnitrate; and, finally, *d*, with 205·81 × 6 = 1234·86, forming the subnitrate at a maximum.* As these subnitrates, or at least the difference between them, have been hitherto unknown to chemists, I shall here give a sketch of their properties and composition.

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* It is proper to observe, that a subnitrate in which the acid is combined with four times 205·81 of oxide cannot exist, because in that case the oxygen of the oxide would be a fractional part of that of the acid.

1. *The First Subnitrate, or Subnitrate at a Minimum,* is produced when nitrate of lead is precipitated by a smaller quantity of ammonia than is sufficient to neutralize one-half of the nitric acid. It is composed of such proportions that the acid contains three times as much oxygen as the oxide. It contains no water of combination. When decomposed by heat it leaves its oxide in an agglutinated and reddish coloured mass, just as happens when the neutral nitrate is treated in the same way. It is very soluble in water, and boiling water dissolves a much greater quantity of it than cold water. On cooling it deposites the salt in small transparent crystals, which decrepitate strongly when heated.

2. *The Second, or Intermediate Subnitrate,* is formed when the neutral nitrate of lead is mixed with a quantity of ammonia exactly sufficient to neutralize $\frac{2}{3}$ of the nitric acid. When more or less of the alkali is employed there is formed a mixture of the intermediate subnitrate with one or other of the two other subnitrates. The intermediate subnitrate is composed in such a manner that the acid contains twice as much oxygen as the base, while the water of combination contains half as much oxygen as the base. When heated it loses its water and becomes yellowish, but recovers its white colour again on cooling; so that it contains enough of acid to conceal the colour of the oxide. It is feebly soluble in pure water; but the addition of any other salt, even nitrate of lead, precipitates it again. When heated to redness it is decomposed, leaving the oxide of lead in the state of a very minute powder, and of a very fine yellow colour.

3. *The Third Subnitrate, or Subnitrate at a Maximum,* is obtained when nitrate of lead is mixed with a considerable excess of concentrated ammonia. It is so constituted that the acid and oxide contain equal quantities of oxygen. It contains water of combination, the oxygen of which is $\frac{1}{4}$ of that of the oxide. It loses its water at a moderate heat, and becomes of a deep yellow colour, which it retains when cold. It is scarcely soluble in water. When decomposed by heat it leaves its oxide in a fine powder and yellow colour, like the preceding subnitrate.

The anomaly in my first experiments having disappeared, and the only circumstance in which these experiments did not coincide with those of Davy and Dalton being removed, it remains only to verify by direct experiment their determination of the quantity of azote contained in nitric acid. My method of proceeding was as follows:—

I dissolved in water in a convenient apparatus 12·05 parts of nitrate of barytes, previously reduced to powder and strongly dried. With this solution I mixed concentrated muriatic acid, and then added 9½ parts of copper. The solution of the copper was promoted by the application of heat, and at the end of six hours it was all dissolved. The liquor had assumed a brownish blue colour, holding in solution a little of the muriate of copper (*murias cuprosus*). The evolution of nitrous gas continued still four hours longer, and the liquid gradually assumed a green colour. After sixteen hours the apparatus was allowed to cool. When the greenish liquid was mixed with distilled water it became milky, in consequence of the precipitation of the *murias cuprosus*; but the precipitate subsided very slowly, and was for the most part (as usually happens) converted into *murias cupricus* before it could be collected, dried, and weighed. To judge from appearances, its weight could not have amounted to one part. Its presence, however, shows that the nitric acid was entirely decomposed. The nitrous gas, examined by means of muriate of iron, left so small a quantity of unabsorbed residue, that we can only ascribe it to the azotic gas disengaged from the water in the pneumatic apparatus.

Let us examine the result of this experiment: 12·05 parts of nitrate of barytes contain 5·00 parts of pure nitric acid, which (supposing the determination of Davy and Dalton accurate) is composed of 2·207 oxygen and 2·793 of nitrous gas: but if the determination of Gay-Lussac be the most correct, the five parts of acid are composed of 1·738 of oxygen and 3·262 of nitrous gas. According to the first supposition the 5·00 of acid will be capable of peroxidizing 9·12 parts of copper, while according to the second they will only peroxidize 7·00 parts of that metal: but in the experiment above described 9·5 parts of copper had been dissolved, while at the same time a small quantity of muriate of copper had formed, owing to the surplus of copper dissolved. We cannot attribute the difference between 7 and 9½ to the formation of muriate of copper, because in that case the quantity of muriate deposited would have amounted to 7½ parts, which is much greater than the truth.

Experiments prove, then:—

1. That azote cannot be considered as the radicle of nitric acid; but that the acid must contain another radicle, the weight of which must be 11·72 per cent. of the acid.

2. That nitric acid does not contain more than 26·43 of azote to 73·57 of oxygen, nearly the numbers established by Davy in his *Elements of Chemical Philosophy*.

3. That the acid containing only 11·72 per cent. of radicle, but 26·43 per cent. of azote, it follows that 11·72 of radicle united to 14·71 of oxygen constitutes azote. Hence azote is composed as follows:—

Radicle	44·32	79·64	100·00
Oxygen	55·68	100·00	125·51

Now this composition approaches very nearly to what I had obtained in my preceding memoirs on this subject.

4. That nitric acid is composed as follows:—

Radicle	11·72	Azote	26·43
Oxygen	88·28	Oxygen	73·57
	100·00		100·00

That is to say, that if we consider it as having azote for its radicle it neutralizes a quantity of base containing $\frac{1}{5}$th of the oxygen in the acid; but if we consider it as having a particular radicle it neutralizes a quantity of base, the oxygen of which amounts to $\frac{1}{6}$th of that in the acid.

I shall now show that the radicle of azote is not, as I supposed for some time, the metallic body conceived to constitute the radicle of ammonia (or at least in the present state of our knowledge we cannot consider it as such); but another body, for which we must have a name in order to avoid circumlocutions. I propose, according to the principles explained in my essay on chemical nomenclature (*Jour. de Physique*, 1811), to give it the name of *nitricum*; as we say *caloricum*, *carbonicum*, &c. The word *nitrogenium*, though sanctioned by two words of the same kind, cannot well be employed, because it has always had the same meaning with *azote*. *Azote*, or *nitrogen*, according to my nomenclature, is the *suboxide of nitric*, *suboxidum nitricum*, just as carbonic oxide gas is the *suboxide of carbon*; that is to say, that both belong to a class of oxides incapable of combining with other oxides till they have united with a greater proportion of oxygen.

Article VII.

An Account of the Explosion of Inflammable Air which lately occurred in the Collingwood Main Colliery. Drawn up for the "Annals of Philosophy."

On Saturday the 17th of July, at two o'clock, p. m. in the Collingwood Main Colliery, situated upon the river Tyne, near North Shields, a very considerable quantity of inflammable air, or carbureted hydrogen gas, came into contact with the pitmen's candles, which caused a most tremendous explosion, by which eight persons were killed upon the spot, and two severely wounded and scorched. The following particulars of this melancholy disaster, were communicated verbatim at the above-mentioned colliery, a few days after the accident, to the writer of this, by Henry Hall, who fortunately escaped, though in the midst of imminent danger.

At the time when the explosion took place the above-named Henry Hall, and five other pitmen, were proceeding with burthens of timber through the old workings or excavations (the proper road being obstructed by a creep,*) in the full confidence of safety, having been assured by Mr. Hope, the under viewer, that there was no fear of the "mine firing." In an instant this young man, Henry Hall, and the five pitmen who were with him, were by the explosion thrown upon their faces; and the shock was so great as to deprive him of sensation, as well as volition, till the after-blast, or after-damp,† as it is called, gave him such excitement that he faintly recollects being urged like a ball along the floor of the mine with incredible velocity. Soon after this he was again deprived of sensation, in which state he continued for about twenty minutes, till he breathed the pure atmospheric air upon the bank, at the top of the shaft, to which place his brother had carried him, who descended into the mine as soon as he possibly could, upon hearing the explosion, at the risk of his own life, for the purpose of saving that of his brother, or of any other person whom he could find. I may remark, by the by, that according to the rules of the Royal Humane Society, if my memory serves me, H. Hall's brother, and several others, are entitled to rewards from that very excellent institution. H. Hall reports, that after he recovered sensation he felt his whole body racked with pain, the burnt places giving him no uneasiness, comparatively speaking; and that his suffering continued without intermission for two days. Bad as H. Hall's case was, the other five pitmen who were with him had not even such an escape, for four of them were instantly killed, and Ralph Stokell so dangerously bruised and burnt in several places that his life was for some time despaired of.

At a distant part of the mine, where some other pitmen were employed in taking up metal plates, timber, &c. Mr. Hope, the under viewer, Mr. Wild, the overman, and two pitmen, were suffocated by the choak-damp, or carbonic acid gas. Mr. Wild had wandered at least a hundred yards before he met his death by suffocation.

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* In working the coal, the pitmen leave pillars, in the form of parallelograms, for the support of the roof. If these pillars are narrow, and the floor of the mine soft or tender, they are apt to sink into the floor, and cause such an approximation as to prevent ventilation, &c. This is technically called a *creep*.

† Vide Annals of Philosophy, vol. i. p. 359.

Upon an explosion taking place in a coal-mine the choak-damp is very rapidly driven through all parts of the colliery from those places where it had accumulated, and the explosion is always followed by another commotion, of a still more dangerous nature, viz. the " back draught," as the miners term it. The back draught is that impetuous current of air which rushes most violently from all sides within the mine, like " the voice of mighty thunderings," to the spot where the explosion occurred, so as to overcome the vacuum which had been effected by means of the explosion.

The following is a list of the persons who were killed :—

Mr. William Hope, under viewer, leaving a wife and four children.

Mr. Ralph Wild, overman, a wife and four children.

James Campbell, pitman, a wife and child.

Ralph Hope, pitman.

Robert Clark, pitman.

Thomas Miller, pitman.

George Richardson, and

William Richardson, pitmen: these two young men were brothers; and, having lost their parents, they had the filial goodness to support their grandmother, now in her 103d year, by their industry.

By the choak-damp a considerable number of horses were suffocated. In this melancholy list the dreadfully uncertain state of the pitmen is clearly demonstrated. Poor Mr. Hope, the under viewer, was heard to exclaim, in astonishment or despair, a moment before his dissolution, " God have mercy upon us; the pit has fired ! " Besides the sufferers, there were 14 or 15 men in the pit, who, as if by a miracle, were saved. They had been employed in a distant part of the colliery; and after the explosion wandered on in darkness and stupefaction till by good fortune they chanced to arrive at that part of the mine where there was a sufficient proportion of atmospheric air to support respiration.

The frequency of these destructive explosions, upon the score of humanity, is greatly to be deplored; the loss to the coal-owners is always very considerable—sometimes enormous; and as I am given to understand there is at length discovered an effectual plan for their prevention, by means of a lamp or lantern invented by Dr. Reid Clanny, which was noticed in the *Annals of Philosophy* for June last, it is greatly to be desired that this method of affording light to the pitmen may come into general use, even where the chance of an explosion is but small; for I am happy to find, upon inquiry, that the expense attendant upon this plan is very trifling indeed, compared with the safety which is insured. Let not habit or usage have any weight here, however praiseworthy under other circumstances; for the loss of valuable lives from explosions is greatly upon the increase, and demands the serious attention of every good man. If this account of the explosion, which is the only one hitherto drawn up, merits a place in the *Annals of Philosophy*, the Editor shall hear from me again upon this most interesting subject.

August, 1813. Φιλελεεινός.

Article VIII.

On the Manner of separating the Oxide of Iron from the Oxide of Manganese. By M. Bucholz.*

M. Bucholz, after having described all the difficulties which chemists experience when they wish to separate with exactness the oxide of iron from the oxide of manganese, adopts the process of Gehlen as the best; which consists in pouring succinate of ammonia into a neutral solution of the two oxides. This process has been confirmed by the subsequent experiments of Klaproth and Bucholz. He wishes only that the succinate of iron were more dense and more easily washed. The process of Berzelius, who employs combinations of benzoic acid for the same purpose, is also good, and may be employed when the experimenter is not provided with succinic acid. Bucholz terminates his paper with an examination of the process published in 1806 by Dr. John, and employed afterwards by Simon. It consists in precipitating the iron from its solution, rendered as neutral as possible by pouring into it a quantity of oxalate of potash. According to John the whole of the iron is thrown down, while the manganese remains in solution. This process, which appears at first sight so advantageous, is at variance with every chemical work, all of which affirm that the oxalate of manganese is nearly insoluble in water.

To put the fact to the test of experiment, Bucholz dissolved carbonate of manganese in acetic acid, and diluted the solution with 16 parts of water. He divided this liquid into two equal portions, into one of which he poured oxalate of potash, and into the other oxalate of ammonia. Both liquids in a short time became muddy, and deposited a considerable precipitate.

M. Bucholz, being afraid that the precipitation might be ascribed to the too great concentration of the liquid, repeated it with some alteration, and found that oxalates precipitate the solutions of manganese not only when diluted with 64 times their weight of water, but also when there is an excess of acid present. The solution of manganese in muriatic acid is even precipitated by the oxalates when diluted with a much greater proportion of water; but the precipitate does not so speedily make its appearance.

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* Abridged from Gehlen's Journal for 1810, p. 673.

These experiments proved that Dr. John had been deceived; but Bucholz wished farther to ascertain if the oxalate of iron was as insoluble in all states as Dr. John had alleged. For this purpose he made the following experiments:—

He took one part of the red muriate of iron which had been suffered to run into deliquescence in the open air, but in a state as nearly neutral as possible. He diluted it with 37 parts of water, and added $\frac{1}{4}$ of neutral oxalate of potash dissolved in 8 parts of water. The precipitate fell much more slowly than when manganese was employed. To observe the phenomena the better this experiment was repeated in the following manner:

One hundred and twenty grains of the same muriate of iron were mixed with an ounce of water, and with an ounce of a solution which contained 80 grains of oxalate of potash, and this mixed liquid was placed in a quiet place. Not a single cloud appeared in the liquid; it merely assumed a brownish green colour, and remained in that state for eight days without undergoing any alteration. Another ounce of the solution of oxalate of potash was then added. The liquid then assumed a deeper brown colour, and in a short time became muddy, by the precipitation of a lemon yellow powder. When this powder was precipitated there gradually formed, in the course of six hours, small crystals of a fine apple-green colour. These crystals formed upon the surface, and were precipitated to the bottom of the liquid. They continued to form during four days, while at the same time a very small quantity of the yellow precipitate attached itself to the vessel in the form of a crust. The liquid, which had still a greenish yellow colour, was decanted. It continued to deposite some green crystals during 15 days, but it was not entirely freed from iron. Having subjected these products to analysis, M. Bucholz ascertained that the yellow powder was oxalate of iron, and the green crystals a triple compound of potash, oxalic acid, and red oxide of iron.

Thus it was demonstrated that the process of Dr. John was a bad one; but to leave nothing undetermined M. Bucholz made the following experiment: He mixed together equal portions of muriate of iron and muriate of manganese, dissolved the mixture in 32 times its weight of water, and poured into the solution neutral oxalate of potash dissolved in eight times its weight of water. There appeared at first a slight precipitate of a yellowish white colour, containing obviously much more oxalate of manganese than oxalate of iron; but this precipitate, as it increased in quantity becoming always more and more yellow, was in a short time much richer in iron than in manganese. This being exactly the opposite of the statement of Dr. John, it follows that he must somehow or other have fallen into an error.

M. Bucholz conceives that the triple salt is formed in the following manner: The muriate of iron containing always an excess of acid, this excess acts upon the alkali of the oxalate, and reduces it to the state of a superoxalate; but this last salt seizes a portion of the oxide of iron, and thus forms the triple salt. Bucholz was surprised at the fine green colour of this salt, which it retains notwithstanding the red oxide of iron, which forms one of its constituents, and which usually gives a brown or yellow colour to those bodies with which it unites.

These crystals assume the form of a flat four-sided prism, the extremities of which terminate in two oblique facets. The taste of this salt is sweetish, and slightly astringent. It dissolves readily in water, and the solution has a greenish yellow colour. The pure alkalies precipitate the iron in the state of red oxide. When calcined there remains a considerable alkaline residuum. This, combined with its other properties, leaves no doubt about its composition.

ARTICLE IX.

Memoir on the Determination of the Specific Heat of the different Gases. By MM. F. Delaroche, M.D. and J. E. Berard.

(*Continued from p.* 219.)

SECTION II.

Determination of the Specific Heat of some Gases, that of Air being considered as unity.

THE process which we have followed in the experiments that we have made on the different gases being the same for all, and this process having been sufficiently explained in the preceding section, we think it unnecessary to enter into a detailed account of the experiments which we made on each particular gas; we think it sufficient to present the results of them in the following table.*

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* The hydrogen gas which we employed was obtained by dissolving zinc in diluted sulphuric acid; the carbonic acid gas, by dissolving white marble; the oxygen, by distilling hyperoxymuriate of potash; the gaseous oxide of azote by decomposing nitrate of ammonia; olefiant gas, from a mixture of alcohol and sulphuric acid; carbonic oxide gas, by heating dry white marble mixed with charcoal exposed to a strong heat.

Names of the gases.		Number of cubic inches of gas that passed thro' the calorimeter in ten minutes.	Pressure and temperature under which the gas passed through the calorimeter.		Number of cubic inches of gas reduced to the temperature of 32°.	Temperature			Degrees of heat lost by the gas in passing thro' the calorimeter.	Excess of the heat of the calorimeter above the ambiant air, minus 4·5°, the effect of the tube.	Ditto obtained by calculation, supposing the current the same as in exper. 1st, and that it lost the same number of degrees in passing thro' the calorimeter.	Ditto supposing the gas had been subjected to a pressure of 29·922 inches of mercury.	Means.
			Therm.	Barom.		At which the gas entered the calorimeter.	Of the calorimeter when at its maximum, and of the gas issuing out of the calorimeter.	Of the air surrounding the calorimeter.					
				Inches.									
Atmospheric air	1 Ex.	2252·5	44·2°	29·154	2196·4	207·7°	77·333°	45·072°	130·347°	27·761°	27·761°	28·159°	28·321°
	2 Ex.	1863·2	52·2	29·717	1788·1	207·1	77·120	49·597	130·019	23·024	28·355	28·483	
Hydrogen	1 Ex.	2309·3	48·0	29·505	2234·8	201·4	76·577	46·472	127·863	25·605	25·657	25·898	25·585
	2 Ex.	1922·4	53·8	29·705	1839·4	204·3	78·453	53·631	125·806	20·322	25·150	25·252	
Carbonic acid	1 Ex.	2203·7	45·0	29·851	2145·7	206·24	81·586	43·794	124·700	33·293	35·624	35·636	35·639
	2 Ex.	1888·8	54·0	29·607	1806·3	203·27	85·276	53·629	121·594	27·148	35·392	35·642	
Oxygen	1 Ex.	2283·6	48·2	29·465	2209·2	207·88	78·384	46·684	129·494	27·200	27·223	27·506	27·657
	2 Ex.	1910·2	54·7	29·540	1824·1	205·34	81·975	55·769	123·365	21·706	27·619	27·808	
Oxide of azote		1849·7	48·2	29·851	1789·4	206·60	82·263	48·061	122·337	29·702	38·223	38·243	38·243
Olefiant gas		1878·4	50·0	29·528	1815·0	207·23	86·518	48·664	120·712	33·154	43·596	43·983	43·983
Carbonic oxide		1878·4	48·2	29·666	1821·1	207·59	76·109	47·255	131·481	24·354	29·119	29·286	29·286

Columns 1, 2, 3, 5, 6, 7, 8, and 9, of this table, point out the circumstances in which the experiments were made, or the result of the experiments, and require no explanation.

The gases which passed through the calorimeter having been measured at temperatures somewhat different, we have in the fourth column, in order to make them more capable of comparison with each other, brought the numbers of the second column to what they would have been if we had measured all the gases at the freezing temperature.

The sixth column indicates the stationary temperature to which each current of gas brought the calorimeter. The reader will recollect that we determined this temperature by means of two series of observations, one in which the calorimeter being a little below this stationary temperature, the rate of the elevation of its temperature was observed; another in which the calorimeter being a little elevated above this temperature, the rate of its cooling was equally observed. To form an idea of the way of obtaining this determination, the reader may consult Note 2 at the end of this paper, in which are given the details of the first experiment made upon atmospherical air.

The numbers composing the tenth column have been calculated on the supposition that the effect produced upon the calorimeter was proportional to the quantity of gas which passed through it, and to the number of degrees of heat lost by the gas —a supposition that cannot be disputed when it relates to so small differences.

The numbers composing the eleventh column were calculated by means of a formula, which will be explained in Section V, which contains the experiments made upon the specific heat of air subjected to different pressures.

This last column, expressing the stationary temperature at which each current maintained the calorimeter, the circumstances being exactly the same, we are entitled to conclude, from the principles which we have explained, that the numbers which it contains are proportional to the specific heat of the gases. Hence the specific heat of atmospheric air being 1·000, that of the gases examined is as follows:—

	Of the same bulk.	Of the same weight.
Air	1·0000	1·0000
Hydrogen	0·9033	12·3401
Carbonic acid	1·2583	0·8280
Oxygen	0·9765	0·8848
Azote	1·0000*	1·0318

---

* The specific heat of azotic gas was not determined by a direct experiment. We have considered it as equal to that of the same bulk of atmospherical

	Of the same bulk.	Of the same weight.
Gaseous oxide of azote	1·3503	0·8878
Olefiant gas	1·5530	1·5763
Carbonic oxide	1·0340	1·0805 *

(*To be continued.*)

Article X.

Account of a singular Effect of Voltaic Electricity on a slender Platina Wire. By George John Singer, Lecturer on Experimental Philosophy.

(To Dr. Thomson.)

SIR,

During a series of experiments on the effect of various mediums on the ignition of platina by voltaic electricity, a wire of that metal $\frac{1}{200}$th of an inch diameter and three inches long was extended in the centre of a globular receiver, which contained about 22 cubic inches of hydrogen gas.

The wire was then placed in the circuit of a voltaic battery of such power as (it had been previously ascertained) would produce a white heat on a similar length when exposed in the open atmosphere.

On completing the circuit there was not any appearance of ignition, but the wire vibrated strongly, and suddenly exhibited a most extraordinary result; near two inches of its length being split into a bundle of minute fibres, irregularly diverging from a central thicker portion, and continuing attached to the remaining wire at each extremity, so as to assume the form of a lengthened spheroid, corresponding in appearance to an electrified bundle of threads having their ends fastened together. The fibres were so minute as to be scarcely visible to the unassisted eye, when viewed separately. One of these being compared under a microscope with a fine wire drawn by Dr. Wollaston's process, was found to have a diameter of less than the 5000th part of an inch.

In the course of innumerable experiments on the fusion of wires by common electricity, I have observed no similar effect;

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air; because it constitutes $\frac{4}{5}$ths of air, and because the specific heat of oxygen differs but little from that of air.

* To calculate the specific heats of the same weights of the gases we have employed the specific gravity of olefiant gas given by Saussure in the Ann. de ...n. lxxviii. 57, and the specific gravity of the other gases given in the Mem. 253.

but I am informed by Mr. Cuthbertson that he has occasionally splintered iron wires; and I find that Mr. Brook noticed the same circumstance with steel wire, in two experiments out of 73, published in the year 1797. The splinters he produced are described as about $\frac{1}{10}$th of an inch long.

I shall not at present speculate on this phenomenon, which seems to prove, by the expansive effect produced, that in certain cases at least electricity passes through the substance of solid matter; and in such passage displays most unequivocally the action of a material power.

I remain, Sir, your's, &c.

Princes-street, Cavendish-square, G. J. Singer.
August 9, 1813.

Article XI.

On the Daltonian Theory of Definite Proportions in Chemical Compounds. By Thomas Thomson, M.D. F.R.S.

(*Continued from* p. 171.)

We have no data for determining the composition of the phosphurets, those only excepted which have been given in a preceding part of the table. No metallic carburets are known to exist. There can be little doubt that plumbago is in fact a pure charcoal, and that the small quantity of iron which it contains is only accidentally present. A carburet of iron, supposing it composed of an atom of each element, would consist of

Iron	89·875
Carbon	10·125
	100·000

But we are not acquainted with any such compound. Messrs. Allen and Pepys found that 100 parts of plumbago, when burnt, left a residue of 5 parts. If we suppose this residue to be peroxide of iron, it will be equivalent to 3·45 parts of iron. According to this statement the plumbago consisted of 96·55 parts of charcoal and 3·45 parts of iron. This amounts to about 248 atoms of carbon combined with 1 atom of iron. We may be quite certain that so great a number of atoms of carbon never could come in contact with one atom of iron, and that therefore such a compound cannot exist: besides, we are not sure that this residue consisted of oxide of iron. Schräder, in an analysis of plumbago inserted in the *Annals of Philosophy*, vol. i. p. 294, has shown that this residue is of a complicated nature, consisting of oxide of iron, oxide of titanium, silica, and alumina.

From Mr. Mushet's experiments it seems to follow that the compound of iron and carbon, the hardness of which is a maximum, the colour white, and the texture crystallized, is a compound of about 7 atoms of iron and 1 of carbon. Steel, if any confidence can be put in the experiments hitherto made to determine its composition, seems to consist of 1 atom of carbon united with from 10 to 13 atoms of iron, according to the nature of the steel: but these chemical analyses are still too imperfect to permit us to place the different varieties of steel and cast-iron in our table.

I shall proceed therefore to the salts, many of which have been analysed with great care, and which constitute the best established department of chemical science.

Genus I.—*Sulphates.*

Sulphuric acid, it will be seen from our table (*Annals of Philosophy*, vol. ii. p. 44), is composed of 1 atom of sulphur and 3 atoms of oxygen, and the weight of an integrant particle of it is 5·000.

		Number of atoms.	Weight of an integrant particle.
164.	Sulphate of potash	$1\,s + 1\,p$	11·000 [a]
165.	Supersulphate of potash	$2\,s + 1\,p$	16·000 [b]
166.	Sulphate of soda	$1\,s + 2\,so$	20·764 [c]
167.	Sulphate of ammonia	$1\,s + 2\,a$	7·284 [d]

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[a] I found by a careful analysis of sulphate of potash, fused previously in a platinum crucible, that 100 parts of it contained 42·2 of acid and 50·1 of potash. Now 5 : 6 :: 42·2 : 50·62; which corresponds with the analysis very nearly. In giving the weight of an integrant particle of the salts I have been obliged to omit the water of crystallization, because it has been accurately determined in a very small number of salts only.

[b] Dr. Wollaston has shown that in this salt the quantity of acid is just double what exists in the sulphate.

[c] According to Wenzel, sulphate of soda is composed of 100 acid + 78·32 base; according to Berzelius, of 100 acid + 79·34 base. Now 5 : 7·882 :: 100 : 157·64, and $\frac{157{\cdot}64}{2} = 78{\cdot}82$. Now this is the mean of the two experiments.

[d] According to Berzelius (*Gilbert's Annalen*, xl. 282), this salt is composed of 100 acid + 42·561 ammonia. Now 100 :

		Number of atoms.	Weight of an integrant particle.
168.	Sulphate of magnesia	1 s + 1 m	7·368 [e]
169.	Sulphate of lime	1 s + 1 l	8·620 [f]
170.	Sulphate of barytes	1 s + 1 b	14·731 [g]
171.	Sulphate of strontian	1 s + 1 str	11·900 [h]
172.	Sulphate of alumina	1 s + 1 a	7·136 [i]
173.	Subsulphate of alumina	1 s + 2 a	9·272
174.	Sulphate of yttria	1 s + 1 y	13·400 [k]
175.	Sulphate of glucina	1 s + 1 g	8·600
176.	Sulphate of zirconia	1 s + 1 z	10·656
177.	Alum	4 s + 2 a + 1 p	30·272 [l]

42·561 :: 5 : 2·128, and $\frac{2·128}{2} = 1·064$, which differs but little from the weight of an atom of ammonia.

[e] According to Dr. Henry, sulphate of magnesia is composed of 100 acid + 47·36 base. This exactly agrees with the statement in the table: nor could it be otherwise, as the weight of magnesia was estimated from that analysis. Berzelius makes it 100 acid + 50·06 base. (*Gilbert's Annalen*, vol. xl. p. 256.)

[f] This coincides almost exactly with the analysis of Berzelius. He found sulphate of lime composed of 100 acid + 72·41 base. Now 5 : 3·620 :: 100 : 72·40.

[g] This coincides with the analysis of Berzelius, who found sulphate of barytes composed of 100 acid + 194 base. Now 5 : 9·731 :: 100 : 194·62.

[h] The weight of strontian was deduced from the supposition that the sulphate of strontian is composed of 100 acid + 138 base. Of course the number in the table is conformable to that supposition.

[i] According to Berzelius, sulphate of alumina is composed of 100 acid + 42·722 alumina. (*Gilbert's Annalen*, vol. xl. p. 262.) Supposing the salt composed as in the table, the number representing an atom of alumina should be 2·136.

[k] Neither this nor the two following salts have been hitherto analysed; but I have inserted them in the table, stating their composition from the very probable analogy that they are composed of 1 atom of acid united to 1 atom of base; this being the case with all the preceding neutral salts in the table, except the sulphate of soda.

[l] This seems to be a combination of an integrant particle of three different salts: namely, 1st. Sulphate of potash, composed of 1 s + 1 p. 2d. Sulphate of alumina, composed of 1 s + 1 a. 3d. Supersulphate of alumina, composed of 2 s + 1 a. These

	Number of atoms.	Weight of an integrant particle.
178. Sulphate of potash-and-ammonia	$2s + 1p + 1a$	17·142[m]
179. Sulphate of potash-and-magnesia	$3s + 1p + 2m$	25·736[n]
180. Sulphate of soda-and-ammonia	$7s + 1so + 6a$	57·616[o]
181. Sulphate of soda-and-magnesia	$4s + 1so + 3m$	42·868[p]
182. Sulphate of magnesia-and-ammonia	$3s + 2m + 1a$	20·878[q]
183. Supersulphate of copper	$2s + 1c$	20·000[r]

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added together constitute the atoms given in the table: so that if preceding analyses be correct, this is the most complicated salt known.

[m] I conceive the triple salts to be formed by the union of an integrant particle of one salt with an integrant particle of another. The present salt consists of an atom of sulphate of potash united with an atom of sulphate of ammonia. If this be correct it should contain 60 parts sulphate of potash and 33·5 sulphate of ammonia. According to Link, it contains 60 sulphate of potash and 40 sulphate of ammonia. No great difference, if we consider the difficulty of the analysis.

[n] According to Link, this salt contains 3 parts of sulphate of potash and 4 parts of sulphate of magnesia. Hence it consists of 1 atom sulphate of potash and 2 atoms sulphate of magnesia.

[o] According to Link, this salt consists of 5 parts sulphate of soda and 9 parts of sulphate of ammonia. If so it is a compound of 1 atom sulphate of soda and 6 atoms sulphate of ammonia.

[p] According to Link, this salt consists of 5 parts of sulphate of soda and 6 parts of sulphate of magnesia. If so it must be a compound of 1 atom of sulphate of soda and 3 atoms of sulphate of magnesia.

[q] According to Fourcroy, this salt is composed of 68 parts of sulphate of magnesia and 32 parts of sulphate of ammonia. If so it must be a compound of 2 atoms of sulphate of magnesia and 1 atom of sulphate of ammonia.

[r] As an atom of sulphuric acid weighs 5, and an atom of peroxide of copper weighs 10, this salt ought to be composed of equal weights of acid and base. Now according to Proust it consists of 33 acid + 32 oxide; according to Berzelius, of 49·1

	Number of atoms.	Weight of an integrant particle.
184. Sulphate of copper	1 *s* + 1 *c*	15·000 [s]
185. Subsulphate of copper	1 *s* + 2 *c*	25·000 [t]
186. Supersulphate of iron	2 *s* + 1 *i*	18·666 [u]
187. Sulphate of iron	1 *s* + 1 *i*	13·666 [x]
188. Subsulphate of iron	2 *s* + 3 *i*	35·999 [y]
189. Persupersulphate of iron	3 *s* + 1 *i*	24·666 [z]
190. Sulphate of lead	2 *s* + 1 *l*	37·974 [a]
191. Sulphate of zinc	1 *s* + 1 *z*	10·315 [b]

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acid + 50·9 oxide. The mean of this gives us almost exactly the numbers in the table.

[s] This salt is given from analogy, never having been analysed.

[t] According to Proust, it is composed of 18 acid and 68 oxide. Now 5 : 20 :: 18 : 72. This approaches pretty nearly to the number found by Proust.

[u] According to the analysis of Berzelius, this salt is composed of 100 parts acid + 88 oxide of iron. Now 5 × 2 = 10 : 8·666 :: 100 : 86·660. This is within $1\frac{1}{3}$ per cent. of Berzelius' analysis.

[x] We are not acquainted with this salt. Supposing it to exist, its composition must be as stated in the table.

[y] The numbers in the table correspond nearly with the analysis of this salt by Berzelius. (*Ann. de Chim.* lxxviii. 223.) According to him, it is composed of 100 acid + 266 oxide. Now 100 : 266 :: 5 × 2 : 26·6; and $\frac{26\cdot6}{3} = 8\cdot866$, a number nearly agreeing with the weight of deutoxide of iron. At the same time I entertain some suspicions of the accuracy of Berzelius' experiment.

[z] I distinguish the salts containing peroxides by prefixing the syllable *per* to the name. The persupersulphate of iron means a salt composed of sulphuric acid and peroxide of iron with excess of acid. The result in the table is derived from the analysis of Berzelius. He found it composed of 100 acid + 65·5 peroxide. Now 100 : 65·5 :: 5 × 3 = 15 : 9·825. And 9·825 differs very little from 9·666, the weight of an atom of peroxide of iron.

[a] According to the analysis of Berzelius, sulphate of lead is composed of 100 acid + 280 yellow oxide of lead. (*Ann. de Chim.* lxxvii. 83.) Now 100 : 280 :: 5 × 2 = 10 : 28. And the weight of an integrant particle of yellow oxide of lead, according to the table, is 27·974.

[b] I found sulphate of zinc to contain 25·8 acid + 28·2 oxide. Now 5 (the weight of an atom of acid) : 5·315 (the weight of

		Number of atoms.	Weight of an integrant particle.
192.	Sulphate of mercury	1 s + 1 m	31·000 [c]
193.	Persulphate of mercury	1 s + 1 m	32·000 [d]
194.	Sulphate of silver	1 s + 1 si	18·618 [e]
195.	Sulphate of bismuth	1 s + 1 b	14·994 [f]
196.	Sulphate of nickel	1 s + 1 n	10·623 [g]
197.	Sulphate of cobalt	2 s + 1 c	19·326 [h]
198.	Sulphate of manganese	2 s + 1 m	19·130 [i]

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an atom of oxide of zinc) :: 25·8 : 27·425. This differs but little from the result which I obtained.

[c] According to Berzelius (*Lärbok i Kemien*, ii. 355), this salt is composed of 16 acid + 84 oxide. Now 5 : 26 :: 16 : 83·2. Hence the composition of the salt is obvious.

[d] These numbers are given from theory. We have no good analysis of *turpeth mineral*. If the numbers in the table be correct, it ought to be a compound of

Sulphuric acid	15·625
Peroxide of mercury	84·375
	100·000

Now Berzelius gives us 12 acid + 88 oxide (*Lärbok i Kemien*, ii. 364), which does not differ very much from our numbers. The composition of the other mercurial sulphates has not been determined.

[e] According to Berzelius, this salt is composed of 25·78 acid + 74·22 oxide. Now 5 : 13·618 :: 25·78 : 70·214. So that the numbers in the table approach very closely to the analysis of Berzelius.

[f] According to Lagerhjelm, sulphate of bismuth is composed of 33·647 acid + 66·353 oxide. Now if we suppose it a compound of one integrant particle of acid and one of oxide its composition would be 33·347 acid + 66·653 oxide.

[g] The best analysis of this salt that we have is that of Tupputi. According to him, it is composed of 53·4 acid + 46·6 oxide. The numbers in the table suppose its constitution to be 47·058 acid + 52·942 oxide. Nearly the inverse of Tupputi's analysis.

[h] According to the analysis of Rolhoff, this salt is composed of 52·11 acid + 47·89 oxide. Supposing it to consist of 2 atoms acid and 1 atom oxide, its constituents would be 51·744 acid + 48·256 oxide. This differs but little from the analysis.

[i] According to John, this salt is composed of 52·06 acid + 47·94 oxide. Supposing its constitution as stated in the table, it

		Number of atoms.	Weight of an integrant particle.
199.	Sulphate of uranium	1 *s* + 1 *u*	20·000[k]
200.	Persulphate of platinum	2 *s* + 1 *p*	24·161[l]

These 37 are all the sulphates which I can venture to insert into the table. The remaining sulphates have been so imperfectly examined that we are still ignorant of the proportions in which their constituents unite. I think there is reason to believe that neither the oxides of arsenic, tungsten, molybdenum, nor chromium, combine with sulphuric acid so as to form sulphates. The oxides of these metals possess properties more analogous to acids than to bases. I think the same observation applies to the oxides of antimony. Probably a sulphate of tin exists; but it has never been analysed. The same observation applies to the sulphate of gold. The other metals, namely, *palladium*, *rhodium*, *iridium*, *osmium*, *tellurium*, *columbium*, and *cerium*, are still too scarce to expect an accurate knowledge of the salts which their oxides are capable of forming.

We have it now in our power to examine into one of the canons advanced by Berzelius, and upon which he has laid a great deal of stress in some of his late chemical dissertations. His canon is, that when two bodies containing oxygen combine, the quantity of oxygen in each is equal, or the quantity of oxygen in the one is twice, thrice, four times, &c. as great as the quantity in the other.

The following table exhibits the respective quantities of oxygen in each of the constituents of all the sulphates the composition of which we have just given.

		Weight of oxygen in the acid.	Ditto in the base.
1.	Sulphate of potash	3	1
2.	Supersulphate of potash	6	1

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would be composed of 52·726 acid + 47·274 oxide. A coincidence as near as could be expected.

[k] According to the analysis of Bucholz, sulphate of uranium consists of 22·1 acid + 77·9 peroxide. If the statement in the table be correct, it should be composed of 25 acid + 75 oxide.

[l] According to the analysis of Berzelius (*Lärbok i Kemien*, ii. 428), this salt is composed of 41·223 acid + 58·777 peroxide of platinum. Now if the supposition in the table be true, it ought to consist of 41·389 acid + 68·611 acid; a proportion which almost coincides with the numbers obtained by Berzelius experimentally.

	Weight of oxygen in the acid.	Ditto in the base.
3. Sulphate of soda	3	4
4. Sulphate of ammonia	3	
5. Sulphate of magnesia	3	1
6. Sulphate of lime	3	1
7. Sulphate of barytes	3	1
8. Sulphate of strontian	3	1
9. Sulphate of alumina	3	1
10. Subsulphate of alumina	3	2
11. Sulphate of yttria	3	1
12. Sulphate of glucina	3	1
13. Sulphate of zirconia	3	1
14. Supersulphate of copper	6	2
15. Sulphate of copper	3	2
16. Subsulphate of copper	3	4
17. Supersulphate of iron	6	2
18. Sulphate of iron	3	2
19. Subsulphate of iron	6	6
20. Persupersulphate of iron	9	3
21. Sulphate of lead	6	2
22. Sulphate of zinc	3	1
23. Sulphate of mercury	3	1
24. Persulphate of mercury	3	2
25. Sulphate of silver	3	1
26. Sulphate of bismuth	3	1
27. Sulphate of nickel	3	2
28. Sulphate of cobalt	6	2
29. Sulphate of manganese	6	2
30. Sulphate of uranium	3	3
31. Persulphate of platinum	6	2

By casting our eye over this table we find that the canon of Berzelius holds in all the examples in the table except seven. The first of these is sulphate of soda. I think it is pretty well established that sulphuric acid is a compound of 1 atom of sulphur and 3 atoms of oxygen. While all the experiments hitherto made lead to the conclusion that soda is a compound of 1 atom of sodium and 2 atoms of oxygen. Now if these conclusions be well founded, as I think they are, Berzelius's canon could not hold with respect to sulphate of soda, unless it were a compound of 2 integrant particles of sulphuric acid and 1 integrant particle of soda, which is just the inverse of its real composition, Indeed, if the canon held good, it would follow as a consequence that an integrant particle of sulphuric acid is in all cases incapable of uniting to an integrant particle of a deutoxide. This rule indeed seems to hold with respect to iron, lead, cobalt, manganese, and platinum; for we find the deutoxide of these metals combine not with 1 but with 2 integrant particles of sulphuric acid. Hence it is probable that the sulphate of iron given in the table does not exist.

If we examine the notes under the table of the composition of the salts it will be seen that none of the other six exceptions to the canon of Berzelius can be considered as valid; because they are either stated from mere theory, or they do not agree with the analyses of the salts in question hitherto published.

Upon the whole, then, the sulphates furnish one decisive exception to Berzelius's canon. This canon appears to me at present entirely empyrical. I cannot see any sound reason (different from the result of analyses) that should lead us to adopt it. Hence the knowledge of this exception disposes me at present to reject it; but we shall be able to judge with more correctness after we have examined a few more genera of salts, especially the nitrates, upon the analysis of which Berzelius seems to me to have founded it.

(*To be continued.*)

ARTICLE XII.

Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Magnetical Observations.

Latitude 51° 32′ 40″ North. Longitude West in Time $6''\frac{82}{100}$.

1813.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Aug. 19	8h 35′	24° 15′ 35″	2h 05′	24° 21′ 52″	6h 40′	24° 16′ 49″
Ditto 20	8 58	24 15 31	2 05	24 22 38	6 57	24 17 06
Ditto 21	8 50	24 15 34	2 05	24 24 33	6 45	24 13 01
Ditto 22	8 50	24 16 30	— —	— — —	— —	— — —
Ditto 23	8 55	24 15 01	2 00	24 23 15	— —	— — —
5 Ditto ~~24~~	8 52	24 15 18	— —	— — —	6 55	24 16 35
Ditto 26	8 27	24 15 25	— —	— — —	6 50	24 18 16
Ditto 27	8 35	24 15 25	2 00	24 25 02	6 35	24 17 04
Ditto 28	8 45	24 16 34	2 35	24 24 07	6 40	24 16 48
Ditto 29	8 55	24 16 01	2 00	24 24 25	6 55	24 16 23
Ditto 30	8 50	24 16 13	1 57	24 24 56	6 45	24 15 13
Ditto 31	8 55	24 16 21	2 00	24 23 40	6 15	24 15 51

Mean of Observations in Aug.					
	Morning	at	8h 44′	Variation	24° 15′ 58″
	Noon	at	2 02	Ditto	24 23 32
	Evening	at	7 05	Ditto	24 16 08

Mean of Observations in July.	Morning	at	8h	37'.....	Variation	24°	14'	32"
	Noon	at	1	50.....	Ditto	24	23	04
	Evening	at	7	08.....	Ditto	24	13	56
Ditto in June.	Morning	at	8	30.....	Ditto	24	12	35
	Noon	at	1	33.....	Ditto	24	22	17
	Evening	at	7	04.....	Ditto	24	16	04
Ditto in May.	Morning	at	8	22.....	Ditto	24	12	02
	Noon	at	1	37.....	Ditto	24	20	54
	Evening	at	6	14.....	Ditto	24	13	47
Ditto in April	Morning	at	8	31.....	Ditto	24	09	18
	Noon	at	0	59.....	Ditto	24	21	12
	Evening	at	5	46.....	Ditto	24	15	25

Magnetical Observations continued.

Month.		Morning Observ.					Noon Observ.					Evening Observ.				
		Hour.		Variation.			Hour.		Variation.			Hour.		Variation.		
Sept.	1	8h	50'	24°	17'	09"	2h	33'	24°	21'	30"	—h	—'	—°	—'	—"
Ditto	2	8	50	24	15	17	—	—	—	—	—	—	—	—	—	—
*Ditto	3	8	45	24	18	11	2	00	24	23	11	6	32	24	17	48
Ditto	4	8	50	24	14	58	—	—	—	—	—	6	25	24	16	52
*Ditto	5	8	48	24	14	10	2	00	24	22	15	—	—	—	—	—
Ditto	6	9	05	24	18	00	—	—	—	—	—	6	18	24	17	44
Ditto	7	8	45	24	20	10	2	02	24	21	35	6	05	24	15	30
Ditto	8	8	55	24	16	27	2	02	24	22	20	—	—	—	—	—
Ditto	9	8	50	24	14	12	1	55	24	20	30	6	07	24	14	16
Ditto	10	8	55	24	17	02	1	55	24	22	44	6	10	24	18	06
Ditto	11	8	55	24	16	54	2	10	24	22	00	6	07	24	17	53
Ditto	12	8	50	24	15	26	1	50	24	24	02	6	15	24	17	52
Ditto	13	8	45	24	16	24	2	03	24	22	56	6	20	24	15	17
Ditto	14	8	55	24	15	20	1	40	24	23	55	5	55	24	14	25
Ditto	15	9	00	24	15	40	2	05	24	22	31	6	10	24	15	39
Ditto	16	9	15	24	16	11	—	—	—	—	—	6	15	24	15	23
Ditto	17	9	55	24	13	12	2	10	24	23	03	6	05	24	17	15

In deducing the mean variation for the month of August the observations made on the 1st are rejected, on account of their differing so much from the others. On the 6th September the wind blew very hard from the S.W. and the needles at intervals vibrated from four to five minutes. Does not, therefore, the unsteadiness of the wind arise partly from electricity? It is not every gust of wind that will produce a vibratory motion.

Rain fallen	Between noon of the 1st Aug. Between noon of the 1st Sept.	0·597 inches.

Evaporation between the same periods 2·70 inches.

ARTICLE XIII.

Proceedings of Philosophical Societies.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the French Institute for 1812.

(*Continued from p.* 76.)

ZOOLOGY, ANATOMY, AND ANIMAL PHYSIOLOGY.

M. le Chevalier Geoffroy-Saint-Hilaire, who has examined at various intervals the numerous family of bats, and has made us acquainted with so many interesting species, proposes to give a general table of them. He has prefaced this undertaking with a dissertation on the rank which these singular animals ought to hold among the mammalia. They were long considered as intermediate between quadrupeds and birds. It is equally obvious that they hold an intermediate place between the quadrumania and carnivorous animals. Among the numerous arrangements proposed by naturalists, there are some, as that of Linnæus in his last editions, and that of Brisson, in which the bats are classed along with the quadrumania; in others, as that of Linnæus in his first editions, and that of Klein, they are placed with the small carnivorous animals, or eaters of insects, as the mole and the hedgehog. Some, as Storr and Cuvier, place them at the head of carnivorous animals, before the insect eaters just mentioned, and immediately after the quadrumania; with this difference, however, that Cuvier distinguishes them more particularly, and makes a subdivision of them. Others, as Ray, Blumenbach, Lacepede, and Iliger, constitute them a separate order; and this order is placed by Ray and by Lacepede in some measure out of the arrangement. By Blumenbach between the quadrumania and the other inguicula, at the head of which this naturalist places the *rongeurs*. Finally, M. Iliger places them before the carnivorous animals, at the head of which are placed, as in the arrangement of Cuvier, the devourers of insects.

It is easy to see that all these combinations will depend upon those organs to which each naturalist has paid the greatest attention. Those who have chiefly attended to the skeleton, to the intestines, to the organization of the feet, to the form of the nails, to the grinders, have considered the bats as analogous to carnivorous animals (and this is the opinion at present most followed); while those who have attended only to the fore-teeth, to the position of the mammæ, to the hanging penis, have considered them as analogous to the quadrumania.

M. Geoffroy, in the work of which we have spoken, insists more than usual upon these last relations, to which he thinks sufficient attention has not been paid. He shows particularly that the singular elongation of the anterior extremities, the general tendency of the skin to become excessively wide, and the peculiar properties which are the consequence of this in the bats, both with respect to their sensations and motions, require us to place these mammalia in a separate order; while, at the same time, their striking resemblance to the quadrumania, and to the carnivorous animals, requires that this order should be placed between them. We may look with interest to the subdivision of this order, and to the history of the species, which M. Geoffroy has promised.

M. de la Mark, employed at the Museum of Natural History in teaching every thing which concerns the animals destitute of vertebræ, published, some years ago, the work which serves as a basis to his course. He explains in it, according to his own method, the classes, orders, and genera, of these numerous animals: but as travellers have since discovered many genera and species, as anatomists have more completely explained the structure, and as the meditations of la Mark on the subject have made him discover various new relations among these animals, he has published an abridged table of his course, after his method in its most perfect state, in which he satisfies himself with giving the characters of the greater divisions, and simply enumerating the names of the genera.

He follows in their arrangement the degrees of complicateness, beginning with the most simple animals. Supposing that those which have no visible nerves only move in consequence of their irritability, he calls them *apathic animals*. He gives the name of *sensible animals* to the other animals without vertebræ, and of *intelligent animals* to those which have vertebræ. To his old classes, now well known to naturalists, he adds the *cirrhipedá*, which include the *glands-de mer*, and other analogous animals, and which he places between his annelides and his mollusca; that of the *epizoaines*, or intestinal worms, which he places among his apathic animals; and the *infusoria*, or microscopic animals without visible mouth or intestines. He leaves the *echinodermes* in his *radiaires*, and among the apathic animals, with a greater degree of simplicity than the intestinal worms. We regret that we have not room to notice the other changes introduced by M. de la Mark into his orders, nor the numerous additions which he has made to the list of genera; but naturalists will not fail to look for them in the work itself.

Notwithstanding the success of the anatomical investigations of animals without vertebræ for several years back, there still remained a family in which the fundamental organs were not well known. It is the family called *echinodermes*, which comprehends the star-fish, and other analogous genera. The Class having proposed a prize for the perfecting of this branch of comparative anatomy, it was gained by M. Tiedeman, Professor in the University of Landshut. The memoir of this skilful anatomist makes us accurately acquainted, for the first time, with many particulars respecting the organization of these singular animals. A species of circulation is easily observed between their organs of digestion and those of respiration, without, however, offering a complete double circle. Nor can the branches be followed in the exterior organs, nor in those of motion. It appears even, according to M. Tiedeman, that a quite different vascular system is distributed to those numerous peduncles which in these animals serve for instruments of locomotion.

The organs of respiration differ much in different genera. In the holothuria they represent hollow trees, whose branches fill and empty themselves with water from without, and are interlaced with a vascular net. In the *stars* and urchins the water penetrates immediately into the cavity of the body, and moistens all the parts of it.

This beautiful work, accompanied by plates exquisitely finished by M. Münz, Doctor of Medicine, appeared to the Class to deserve the prize, by the number of new facts well authenticated which it presents, and by the great progress which it has made to the intimate knowledge of the echinodermes, though it has not completely answered the question proposed relative to their circulation.

A family much more simple in its organization than the echinodermes, but much more numerous in species, namely, the *corals*, and other animals composed of a solid basis, has been particularly studied by M. Lamouroux, both with respect to the species and the methodical arrangement. This naturalist has made a great collection of those whose basis is not stony, and which present forms so agreeable, and often so singular; and comparing with much care the form, the mutual position, of the cells from which the polypi issue, and all the other visible differences of these animals, he proposes to add 28 new genera. This is an important work for the perfecting of the system of animals; but it does not, from its nature, admit of an abridged analysis. We are anxious for its speedy publication.

M. Cuvier, proposing soon to begin printing the great book on comparative anatomy with which he has been occupied for so many years, has presented to the Class a table of the divisions according to which the animal kingdom will be distributed in that work. Naturalists have been long struck with the great differences which separate animals without vertebræ from each other, while animals with vertebræ resemble each other in so many respects. Hence a great difficulty in generalizing that branch of comparative anatomy, while it is easy to generalize what relates to the animals with vertebræ. But this difficulty has suggested its own remedy. From the manner in which the propositions relative to each organ are always grouped, M. Cuvier concludes that there exist among animals four principal forms. The first is that which is known under the name of animals with vertebræ; and the three others are nearly similar to it in the uniformity of their respective plans. The author calls them *mollusca*, *articulated animals*, and *radiated animals*, or *zoophites*. He subdivides each of these forms or branches into four classes, from motives nearly similar to those which have produced the four classes generally adopted among the animals with vertebræ. He has drawn from this disposition, in some respects symmetrical, a great facility in reducing under general rules the differences of organization.

The comparison which the same author has made of the osteology of the animals with vertebræ, has given him ideas respecting the bony structure of the heads in this class, which he has likewise presented to the Class.

It had been for some time observed that the oviparous animals with vertebræ, that is to say, birds, reptiles, and fishes, had certain common relations in their structure which distinguished them from the mammalia. M. Geoffroy-Saint-Hilaire had even presented some years ago an elaborate essay on the subject, of which an account was formerly given, in which, among other things, he had shown the identity of the structure of the heads of oviparous animals, and the resemblance in the numerous pieces which enter into their composition, with that of those which we distinguish in the fœtus of mammalia, where, as is known, the bones are much more subdivided than in adults.

M. Cuvier, adopting the views of M. Geoffroy, has tried to determine in an accurate manner to what bone of the head of mammalia corresponds each group of bones in the head of the different oviparous animals; and he conceives he has succeeded, by joining to the analogy of the fœtus of the first the consideration of the position and of the functions of the bones; that is to say, by examining what organs they protect, to what nerves and vessels they give passage, and to what muscles they furnish attachments.

M. Jacobsen, Surgeon-Major in the armies of the King of Denmark, has made known to the Class an organ which he discovered in the nostrils of quadrupeds, with which no anatomist seems to have been acquainted. It consists in a narrow sack placed along the canal of the nose, defended by a cartilaginous production, covered internally by a mucous membrane, doubled in part by a glandular tissue, receiving remarkable nerves, with distinct divisions of the first pair, and which open most commonly into the palate, behind the fore-teeth, by a canal which passes through the hole called *incisive* by anatomists. This organ does not exist in man, and is more distinct in most herbivorous animals than in the carnivorous. We must suppose that it is connected with some of those faculties which nature has given to quadrupeds, but denied to our species, as that of rejecting poisonous substances, of distinguishing the sex, the state of heat, &c.

The particular history of animals is enriched with important works and interesting observations.

M. de Humboldt, Foreign Associate, has published the first volume of his *Observations on the Animals of America*, in which he has inserted not only his different researches on the condor, the electric eel, the crocodile, and many other objects of which we have spoken in our preceding analyses; but he has likewise given several new memoirs, namely, on the apes of the New World, of which Buffon and Gmelin only made known 11 or 12 species, but which Humboldt, uniting his observations with those of Azzara and Geoffroy-Saint-Hilaire, makes 46. He has recently read to the Class another memoir, intended for his second volume, in which he describes new species of serpents that he found in Guyana.

The tempests which agitated the sea last winter threw ashore several large cetaceous fish upon our coasts. The Class appointed, as a commission to examine the facts which were received respecting these animals, MM. le Comte Lacepede, Geoffroy-Saint-Hilaire, and Cuvier.

These naturalists have observed that several of these animals were formerly unknown, and that this subject, which might be interesting to our fisheries and our commerce, deserved to draw the attention of Government. They gave a description of a species thrown ashore in great numbers near St. Brieux. M. Lemaout, naturalist and apothecary in that city, having collected with much care all the essential parts, it was easy to recognise a species of dolphin hitherto unknown to naturalists, and of which there only existed a bad figure in the treatise on fishes by Duhamel. It is distinguished by the globular form of the head, almost similar to an ancient helmet. Its length is about 20 feet.

We noticed last year the researches of M. Lamouroux on the innumerable and very small eels, known at the mouths of some of our rivers by the name of *montée*, and we announced the probability that they might belong to some of the little known species of this genus. M. Lamouroux has determined by new experiments that the montée is the fry of the *pimperneau*, a species of eel noticed by Lacepede in his history of fishes, and which is distinguished from the others by its pectoral fins being hollowed out like the wings of bats.

M. Risso, naturalist at Nice, who published two years ago an excellent work on the fishes of that coast, has just sent another to the Class on the crustaceæ, that is to say, on the animals of the crab family. M. Risso adopts in his arrangement the method of Latreille, to which he adds four new genera. He describes 100 species, about the half of which appear new to him: 16 are represented in coloured plates. The Class, in applauding the zeal with which M. Risso, in a situation so unpropitious, has endeavoured to make known the animals of the Mediterranean, still so little studied, would at the same time have desired more precision in the descriptions, before acknowledging the novelty of so great a number of species.

The ancients speak much of an insect which they call *buprestes*, or *burst-ox*, because, according to them, it made the cattle burst who swallowed it with the grass; but they have given us no detailed description of it. The moderns have applied this name very variously; nor does it appear that any of them has recognised the insect to which it truly belonged. M. Latreille, after a careful comparison of the passages in which the properties ascribed to it are mentioned with what we know at present, thinks that it was probably the *meloe proscarabæus* of Linnæus, or some similar species. The *meloe* are the only insects possessing acrid and suspicious properties that live among the grass, and move so slowly as to be easily swallowed by cattle.

Our associate M. de la Billardiere, who employs himself in bringing up bees, having observed one whose abdomen was larger than usual, found in it a white worm, which M. Bosc examined. The body of this worm was white, divided into 12 rings, flattened below, terminated at one extremity by two large tubercles, pierced each with an oval hole, and at the other by two soft points. Under the tubercles is a transverse slit. M. Bosc, regarding this slit as the mouth, considers the part terminated by two points as that where ought to be the anus; and ranging the animal among intestinal worms, he makes a new genus of it, under the name of *dipodium*. He admits, however, that it is possible that the organs may in fact be reversed, and then the worm would much resemble the larvæ of flies with two wings. There is reason to believe, from the observations of Latreille, that the larva of one of these flies (the *canops ferruginosa*) lives in the interior of the drones. It is very remarkable that so large a worm should inhabit the body of an insect so small as a bee.

The portion of digestion which takes place in the stomach must have early attracted the attention of physiologists, and recourse was had successively to all the powers of nature to explain it. It was long ascribed to the muscular trituration of the stomach; but Reaumur having remarked that food contained in incompressible tubes, open at the two ends, was digested like other food, the general opinion since that time has been that the food was dissolved by means of a juice secreted by the stomach.

Spallanzani, in a very celebrated work, having applied the gastric juice out of the stomach to every kind of food, affirmed that it produced, when assisted by heat, effects nearly similar to those produced in the stomach itself. This philosopher went so far as to ascribe to this gastric juice, thus separated, the property of stopping putrefaction. He drew this conclusion from his observations, which has been tacitly adopted by most physiologists, that the gastric juice produces its effects in consequence of its peculiar nature, of its composition, and affinities.

M. de Montegre, Doctor of Medicine, having the power of throwing up, without inconvenience, what he has in his stomach, has thought of employing this power in order to determine the different points of the received doctrine respecting digestion. When he throws up the contents of his stomach while fasting he obtains a notable quantity of a liquid which he considers as true gastric juice, and which he examined with respect to its chemical properties as well as its action on the food.

He found this liquid very similar to saliva; but its action appeared to him very different from the statement of Spallanzani. When exposed to a temperature similar to that of the human body, in phials placed under the armpit, it putrefied exactly like saliva. It did not stop the course of putrefaction in other substances, except when it was acid; and by adding a little vinegar to saliva it was made to possess the same property. This acidity is not essential; and when M. de Montegre swallowed enough of magnesia to absorb it the digestion went on as well as usual. Acidity appeared again in a little time: even when M. de Montegre mixed the food which he swallowed with magnesia it became acid after a sufficient time.

These experiments, repeated a great number of times, and with all the requisite precautions, have induced the author to conclude that the gastric juice does not differ from saliva, that it cannot stop putrefaction, nor produce digestion independent of the vital action of the stomach; and that the acidity which appears, and which the food evolves during digestion, is an effect of the action of the stomach.

It is much to be wished that M. de Montegre would continue his researches, and make them also upon those animals that Spallanzani employed, that we may determine what to think of a doctrine which has for a considerable time been generally embraced.

That authors may be able to verify the date of their observations, we shall here notice some memoirs which have been presented to the Class, but of which the verification is not yet finished, reserving to ourselves the liberty of returning to them next year, and of making known the opinion which has been formed of them.

M. de Blainville, Joint Professor to the Faculty of Sciences of Paris, has described at full length the forms of articulation of the fore-arm and arm in different animals, and determined the motions which each of these forms makes necessary, chiefly with regard to the greater or smaller facility of rotation. This dissertation, on a point of importance relative to the mechanism of animals is interesting also, as far as regards their classification; for the degree of rotation of the fore-arm having considerable influence on the address of the animals, ought to be considered, as far as regards the degree of perfection, and of course influences their natural affinities.

The same anatomist has presented a memoir on the form of the sternum in birds. As this bone, or rather this great bony surface, resulting (as M. Geoffroy has shown) from the union of five different bones, gives origin to the principal muscles of the bird, the more solid and extended it is, the more solid a point of support does it furnish to these muscles, and the more ought it to contribute to render the flight powerful. It ought therefore to have an influence over the whole economy of the bird, and give useful indications respecting the classification of these animals. M. de Blainville draws his indications from the membranous spaces, more or less extended, which supply the place of bone in a part of the sternum. He adds the consideration of the fork, and of some organs connected with it, and in most cases finds a great agreement between the disposition of these parts and the natural families. However, there exist exceptions so manifest that we cannot entirely confide in this new way of classification.

M. Marcel de Serres, Professor to the Faculty of Sciences of Montpelier, has drawn up a laborious work on the anatomy of insects, and particularly on their intestinal canal, which he has described with much detail in a great number of species. His object was to determine the functions peculiar to the different parts of the canal and its appendages: and, besides his dissections, he has made ingenious experiments on living individuals. Coloured liquors injected into the cavity of the peritoneum were absorbed by long slender vessels, which always adhere to some part of the intestinal canal: hence he conceives that the use of these vessels is to secrete from the common mass of humors digestive liquors, and to throw them into the canal. An attentive examination of certain sacks, which in some genera have been considered as stomachs, in others as cœcums, and the certainty acquired that the food does not enter there, but on the contrary that they are found full of bilious liquor, has induced M. de Serres to conclude that they are reservoirs of that humor.

He deprives the grasshoppers, and the analogous genera, of the quality of ruminating animals, which had been ascribed to them, and he has convinced himself that these animals do not bring the food back to the mouth; but that they throw out only in certain circumstances this biliary juice, of which they have so great a quantity. This long memoir contains many other curious observations on the form of the intestinal canal, the proportions of its parts, and their relation to the disposition of insects. We shall speak of it with detail in our next year's analysis.

M. Dutrochet, physician at Chateau-Renaud, department of the Indre, has made a curious observation on the gestation of the viper. He assures us that the young vipers have their umbilical vessels distributed not only on the yoke of the egg, in which they are at first enclosed, but that a part of these vessels is distributed likewise on the internal surface of the oviducts, and forms a net which may be considered as a real placenta. The vipers in that case would participate in the mode of nutrition peculiar to the mammalia, and in that hitherto conceived to belong exclusively to the whole class of oviparous animals.

(*To be continued,*)

Article XIV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Lectures.*

The following arrangements have been made for lectures, at the *Surrey Institution*, in the ensuing season:—Mr. I. Mason Good, on the Philosophy of Physics; to commence on Friday, the 5th of November, and to be continued on each succeeding Friday.—Dr. Thomson, on Chemistry; to commence on Tuesday, the 9th of November, and to be continued on each succeeding Tuesday.—Mr. Bakewell, on Natural and Experimental Philosophy; to commence early in January, 1814.—Dr. Crotch, on Music; early in February, 1814.

William Thomas Brande, F.R.S. Prof. Chem. R.I. will commence his course of Lectures on Chemical Philosophy at the Theatre of Anatomy, Windmill-street, on the second Tuesday in October, at nine in the morning. They will be continued every Tuesday, Thursday, and Saturday, throughout the season, and will terminate in May. The course is divided into the five following parts:—1. Of the Powers and Properties of Matter, and the General Laws of Chemical Changes. 2. Undecompounded Substances, and their mutual Combinations. 3. Vegetable Chemistry. 4. Animal Chemistry. 5. Geology.

Middlesex Hospital.—The Autumnal Course of Lectures on Midwifery, delivered by Dr. Merriman, Physician-Accoucheur to this Hospital, and to the Westminster General Dispensary, will commence October 11th, at half-past ten o'clock.

On Monday, October 4, a Course of Lectures on Physic and Chemistry will commence, at No. 9, George-street:—On the Therapeutics and Practice of Physic, from eight till nine; and the Chemistry from nine till ten. By George Pearson, M.D. F R.S. A Register is kept of the Cases of Patients in St. George's Hospital, and a Clinical Lecture is delivered every Saturday morning, at nine o'clock.

Mr. T. J. Pettigrew will commence a course of Lectures on Human Anatomy and Physiology, on Wednesday, the 20th of October. The course, to be comprised in 12 Lectures, will be delivered on Wednesday and Friday evenings, at eight o'clock precisely. The Introductory Lecture will consist of a General View of the Animal Structure, to which there will be free admission. Particulars respecting the Lectures may be obtained by application to the Lecturer, No 3, Bolt-court, Fleet-street.

Dr. Prout intends, in the course of the winter, to deliver a series of Lectures on Animal Chemistry. The object of these Lectures will be to give a connected view of all the principal facts belonging to this department of chemistry, and to apply them, as far as the present state of our knowledge will permit, to the explanation of the phenomena of organic actions.

Dr. Thomson proposes next winter to give a Practical Course of Chemistry to a very limited number of young Gentlemen, who will reside in his house during its continuance. The Course will begin on the 1st of January, 1814.

II. *New Example of Combustion during Combination.*

It is well known that when sulphur is made to combine with copper, iron, and some other metals, previously reduced to powder, and well mixed, the compound becomes red hot, and glows like a live coal just at the instant of its formation. The same thing takes place when phosphorus is made to unite with lime, barytes, and strontian. When quicklime is slacked in obscurity, it frequently becomes luminous; and there is little doubt that the same thing would happen to barytes and strontian. Chevreul has lately observed that when barytes or strontian is heated in contact with muriatic acid gas, the gas is absorbed, and the earthy salt formed becomes red-hot. There can be little doubt that this evolution of heat is owing to the condensation of the gas. It is true that in the present case the agency of oxygen is not excluded; for if we adopt the opinion of Davy respecting the composition of muriatic acid, it is obvious that in the present case a double decomposition takes place. The chlorine of the muriatic acid unites with the metallic basis of the barytes, and forms *barytane*, while the hydrogen of the muriatic acid combines with the oxygen of the barytes, and forms water; but if we consider that both muriatic acid and barytes are products of combustion, it will be obvious that the presence of the oxygen alone cannot account for the light and heat evolved.

III. *Camphoric Acid.*

Camphoric acid was discovered by Kosegarten, and afterwards examined by Bouillon Lagrange. Bucholz has lately published the result of his experiments on this acid, which, from the known precision of this chemist, deserve to be stated here.

Camphoric acid is white, and assumes the form of feather-shaped crystals. Its taste is acid, and it leaves an impression of bitterness upon the palate. It dissolves in 100 times its weight of cold water, and in 11 times its weight of boiling water. 100 parts of cold alcohol dissolve 16 of camphoric acid. Boiling alcohol is capable of dissolving any quantity of it whatever. When camphoric acid is sublimed, it is partly decomposed, and the sublimed portion refuses to crystallize. 50 grains of camphoric acid require for saturation 28 grains of carbonate of lime (or 15·7 grains of lime). The camphorate of lime has an excess of acid, and crystallizes so irregularly that the shape of the crystals could not be determined. Its taste is somewhat similar to that of lime, and not the least saline. When heated it does not melt, but is converted into carbonate of lime. 100 parts of cold water dissolve 21·6 of this camphorate.

The camphorate of potash may be obtained in prismatic crystals by concentrating the solution, and setting it aside for spontaneous crystallization. Its taste is aromatic and bitter. When heated it melts in its water of crystallization.

IV. *Church Steeple of Greenwich.*

In consequence of a letter from a correspondent, I was induced to make some inquiry relative to the accident which befel the church steeple of Greenwich some months ago; but the information received exhibited no novelty. The steeple was struck with lightning, and the uppermost part of it, 14 feet in length, was thrown down. It appears that a bar of iron passed from the top of the steeple 14 feet down. Where it terminated, the mischief commenced. The lightning, in making its way down, had torn the building in that place. It got afterwards to some wires connected with the bell, which were melted.

V. *Ulmin.*

Professor Berzelius informs me, in a letter which I lately received from him, that *ulmin* is a vegetable substance, not confined to the genus ulmus, but that it forms a constituent part of the barks of almost all trees. He found it in the bark of the *cinchona officinalis* (*Jesuit's bark*), and in that of the *pinus sylvestris* (*Scotch fir*). It cannot be discovered by the usual mode of analysing bark; because, when the bark is digested in hot water, the ulmin combines with the tannin of the bark, and can be no longer recognised as a peculiar substance. If we begin the analysis by digesting the bark in alcohol, and afterwards in cold water, the ulmin remains undissolved, and may be afterwards obtained by means of hot water, especially if that water holds a little alkaline carbonate in solution. The properties which I found the substance thus obtained to possess agree nearly with those which you assign to ulmin. "If," says Dr. Berzelius, "you think this subject of any importance, I have no doubt that my friend Dr. Young will communicate to you the details of my comparative analyses of these two barks, which you will probably find interesting in other respects besides in those particulars which relate to ulmin."

VI. *Oxides of Gold.*

Professor Berzelius has favoured me with the following observations on the account of the oxides of gold which appeared in the second Number of the *Annals of Philosophy*:—"In your account of the oxides of gold you have done me the honour to quote my Manual of Chemistry on the subject. Though I am much flattered by the circumstance, I must take the liberty to observe, that the numbers which you have quoted as mine have been inaccurately quoted. In my Manual I have said that 100 parts of gold combine with 4 and with 12 of oxygen, omitting the fractions. In Davy's Chemistry the result of the same analyses is added as an appendix, but by an error of the press 11·026 has been printed instead of 4·026.—You have given the description of the oxide of gold from Vauquelin. Yet M. Oberkampf, whose dissertation you likewise quote, has proved by decisive experiments that what Vauquelin considered as an oxide of gold is in fact a submuriate. I have verified this assertion of Oberkampf. The oxide of gold is blackish brown, and is obtained by dropping muriate of gold into a solution of caustic potash in water.—Towards the end of that article, you draw, as a consequence from the experiments of Oberkampf, that the observation which I have made respecting the ratio of the sulphur in the sulphuret, and the oxygen of the oxide of the same metal, may be rejected as inaccurate in consequence of the want of correspondence between my experiments and his. A small ake in the translation, it appears, here occasioned an error on your part. I never made use of the word *protoxide*, which, though ingenious, does not answer all the purposes of a chemical term. You know perhaps that I divide metallic oxides into *suboxides*, *oxides*, and *superoxides*,* and that the class of sub-oxides never could be compared with the sulphurets. As to the pretended want of harmony between the sulphuret of gold of Oberkampf and my analysis of the oxide of gold, I beg you to observe that 12·077 (the oxygen that combines with 100 gold) × 2 = 24·154; but Oberkampf found that 100 gold are combined in the sulphuret with 24·39 of sulphur. Hence the experiments agree sufficiently well.† If, on the other hand, you mean to say that the suboxide of gold contains only 4 oxygen, and that therefore the sulphuret of gold ought to contain only 8 of sulphur; I must observe, that in decomposing the muriate of the suboxide by hydrosulphuret of potash, we would probably obtain a sulphuret corresponding with the suboxide, just as happens when we decompose in that manner the muriate of the suboxide of mercury."

Professor Berzelius concludes his letter as follows:—"As to your intention of checking the propensity of chemists to generalize too much, I approve it exceedingly; but request that you will not extend it to the theory of chemical proportions; because it is either general or null. It rather requires to be supported by your credit, since the disposition of chemists appears to me little disposed to acknowledge its truth. I have been publishing memoirs on the subject on the continent for more than three years, without having hitherto, as far as I know, made a single proselyte. I do not speak of England, where the explanations of Dalton, seconded by Wollaston and by yourself, have begun to acquire some credit."

VII. *Alcohol of Sulphur.*

My chemical readers will learn with pleasure that the *alcohol of sulphur*, or *sulphuret of carbon*, which has of late been made the subject of some curious experiments by Professor Berzelius and Dr. Marcet, is prepared and sold by Mr. William Allen, Plough-court, Lombard-street.

VIII. *Gong.*

While looking over a set of German Journals, which I lately received from the continent by the way of Sweden, I observed an analysis by Klaproth of what he called the gonggong. (*Gehlen's Journal, Second Series*, vol. ix. p. 408.) From the account which he gives of this instrument, and the result of his analysis, there can be no doubt that it was the same as that of which I gave an account in the last Number of the *Annals of Philosophy*. Klaproth found the specific gravity of the gong which he examined 8·815. It was not so heavy as my specimen, the reason of which probably was that it contained more tin; for he found it composed of

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* I shall take a future opportunity of laying Berzelius's chemical nomenclature before the reader.—T.

† I have stated my opinions on this subject at length in the 7th and 8th Numbers of the Annals of Philosophy (pages 32 and 109). Berzelius will there see what the real points are about which we differ in opinion. He will see too that I recognize the agreement which he has here pointed out.—T.

Copper	78
Tin	22
	100

IX. *Extract of a Letter from Dr. Wollaston.*

The following letter was received too late for insertion in our last Number:—

DEAR SIR, *August* 24, 1813.

In the continuation which you have promised of the experiments of Messrs. Delaroche and Berard I hope you will correct a mistake they are under concerning the invention of the gazometer they employ.

I believe that the contrivance which you see ascribed to me (p. 91 of the Ann. de Chimie) was invented at Paris by Messrs. Girard as a means of giving uniform pressure in a hydrostatic lamp, that was brought into this country about two years since.

Believe me ever truly yours,
W. H. Wollaston

Article XV.

List of Patents.

Charles Wilks, of Ballincollig, Cork; for certain improvements in the naves of wheels for carriages, and for centres of wheels for carriages, and for centres for wheels of machinery for various purposes. Dated June 29, 1813.

James Penny, Low Nuthwaite, Lancaster, and Joseph Kendal, Cockershell, Lancaster; for an improved principle or plan for making pill and other small boxes. Dated June 29, 1813.

John Curr, Sheffield; for methods of applying flat ropes to horse-gins, and perpendicular drum shafts of steam engines, for drawing coals, minerals, or water, out of mines, whereby the horses' labour is greatly diminished, and flat ropes working on horse-gins and steam-engines, constructed with the said perpendicular drum shafts, are preserved from material injury. Dated June 29, 1813.

Thomas Tod, Bristol, for a machine on an improved construction for the purpose of separating corn, grain, and seeds, from the straw. Dated June 29, 1813.

Charles Wyatt, Bedford-row, London; for a method of casing or facing brick and other buildings with stone. Dated June 5, 1813.

Ambrose Tickell, Lambeth; for an alarum and machinery for the discovery and detection of depredators in a house or premises. Dated July 1, 1813.

Edward Thomason, Birmingham; for various improvements in the construction of whips. Dated July 3, 1313.

Robert Adams, Holborn, London; for a method of preparing blacking, whereby a higher polish is given, and the leather better preserved. Dated July 7, 1813.

John Millard, Cheapside, London; for a method of manufacturing cotton wool free from mixture into cloth, for the purpose of regulating perspiration. Dated July 14, 1813.

John Clark, Bridgewater, Somerset; for a method of making or constructing beds, pillows, hammocks, cushions, and various other articles of that sort, in a different manner, and of different materials from any hitherto used. Dated July 14, 1818.

Alexander Moody, Southwark, tanner; for a method of tanning or dressing white buff leather. Dated July 14, 1813.

William Godfrey Kneller, Croydon, Surrey, chemist; for a method of manufacturing verdigris of the same quality as is known in commerce by the name of French verdigris. Dated July 14, 1813.

George Ferguson and Joseph Ashton, Carlisle, hatters; for an improved, light, elastic, water-proof hat, commonly called a beaver. Dated July 14, 1813.

Robert Prettyman, Ipswich, Suffolk; for improvements in the pan, touch-hole, and pan-cover, of a gun lock. Dated July 19, 1813.

Richard Pering, Dock-yard, Stoke Demerel, Devon; for an anchor made on new principles; which consists, first, in continuing the grain of iron from the shank into the arms, similar to the shape of a knee or arm of a tree, whereby the necessity of effecting a junction at the crown, as at present welded, is superseded: secondly, in carrying a piece of iron across the crown from the centre of each arm, making thereby a perfect truss, which when welded resembles the form of a truss beam: thirdly, in forming both the shank and arms of flat bars, placed so as to act edgewise on the line of resistance when the anchor is in the ground: and fourthly, in forming the largest part of the shank one-third down from the crown in a line across from toe to toe of the arms. Dated July 23, 1813.

Frederick Koenig, of Castle-street, Finsbury-square, printer; for certain additional improvements on his method of printing by means of machinery. Dated July 23, 1813.

John Lewis, Llanelly, Carmarthen; for certain improvements in the art of smelting copper ore. Dated July 23, 1813.

Charles James Mason, Lane Delph, near Newcastle-under-Lyne, potter; for an improvement in the manufacture of English porcelain. Dated July 23, 1813.

ARTICLE XVI.

Scientific Books in hand, or in the Press.

Mr. Singer has in the Press Elements of Electricity and Electro-Chemistry, including Voltaic Electricity, or Galvanism. The work will contain a variety of original experiments, and an account of a New System of Electrical Insulation recently invented by the Author.

Sir Everard Home is about to publish his Course of Lectures on Comparative Anatomy, delivered at the College of Physicians.

Mr. Dodsworth is preparing for the Press a Description of Salisbury Cathedral, including an account of its Monuments, and Biographical Memoirs of the Bishops, in a Quarto Volume, with about thirty Engravings.

Mr. Clarke proposes to publish an Investigation of the Mechanical Science and Historical Descent of Architecture in England during the middle ages, in a Quarto Volume, with about thirty Engravings.

The Fourth Volume of the Transactions of the Medical and Chirurgical Society of London will be published in October.

Mr. Hopkirk, Fellow of the Linnean Society, and Member of the Wernerian Natural History Society of Edinburgh, is speedily to publish Flora Glottiana—a Catalogue of the Indigenous Plants on the banks of the river Clyde, and in the Neighbourhood of the City of Glasgow.

A New Edition of Sir William Dugdale's History of Embanking and Draining, with a continuation to the present time, is preparing for publication.

⁂ Early Communications for this Department of our Journal will be thankfully received.

ARTICLE XVII.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer.			Thermometer.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
8th Mo.										
Aug. 19	N	30·14	30·05	30·095	71	47	59·0	—		☾
20	N	30·14	30·11	30·125	70	40	55·0	—		
21	N W	30·11	29·83	29·970	68	50	59·0	·26	—	
22	N W	29·91	29·71	29·810	65	44	54·5	—	·17	
23		30·23	29·91	30·070	67	46	56·5	—		
24	N E	30·25	30·23	30·240	69	42	55·5	·24		
25	N E	30·25	30·23	30·240	70	52	61·0	—		
26	N E	30·23	30·15	30·190	69	48	58·5	—		●
27	N W	30·15	30·10	30·125	68	51	59·5	·25	—	
28	N	30·10	30·06	30·080	66	53	59·5	—	·15	
29	N E	30·22	30·10	30·160	69	52	60·5	—	4	
30	N E	30·26	30·26	30·260	67	53	60·0	—		
31	E	30·26	30·05	30·155	70	53	61·5	·32		
9th Mo.										
Sept. 1	S E	30·05	29·85	29·950	65	56	60·5	—	—	
2	S W	29·90	29·85	29·875	67	48	57·5	—	—	☽
3	S	29·95	29·85	29·900	75	58	66·5	—		
4	S	29·85	29·75	29·800	73	60	66·5	—	—	
5	S W	29·75	29·25	29·500	70	56	63·0	—	·69	
6	S W	29·49	29·27	29·380	68	52	60·0	·61	—	
7	W	29·67	29·49	29·580	64	44	54·0	—	—	
8	N W	29·85	29·67	29·760	56	43	49·5	—	—	
9	N W	30·19	29·85	30·020	50	41	45·5	—		
10	N W	30·24	30·19	30·215	61	49	55·0	—		○
11	S W	30·24	30·10	30·170	70	58	64·0	·55		
12	S W	30·06	30·00	30·030	72	51	61·5	—		
13	N W	30·06	30·05	30·055	63	51	57·0	—	·18	
14	N W	30·18	30·06	30·120	64	42	53·0	—		
15	S W	30·18	30·17	30·175	70	51	60·5	—		
16	N W	30·29	30·17	30·230	72	50	61·0	·40		
		30.29	29·25	30·009	75	40	58·44	2·63	1·23	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Eighth Month.—19. *Cirrus* and *Cirrostratus* clouds: rather windy. 20. *Cumulostratus*: windy. *Cirrus* clouds, very red, at sun-set. 21. Windy. 22. Cloudy morning, small rain: showers and wind: rainbow about 6 p. m. the sky richly coloured, and the clouds evaporating. 24. *Cumulus* during the day: *Cirrus* at sun-set: the twilight brilliant and coloured, with traces of *Cirrocumulus* and of *Stratus*. 25. Overcast, with *Cumulostratus*: twilight opake and coloured. 26. Windy, a. m. *Cumulostratus* clouds, the remains of which, at sun-set, glowed with a succession of crimson and purple tints, on a full orange ground. 27. Windy, a. m.: a little rain. 28. Overcast: much wind, and at night rain. 29. *Cumulostratus* clouds chiefly: a shower or two: the twilight luminous, but opake, and surmounted by a blush of red, considerably elevated. 30. a. m. Cloudy.

Ninth Month.—1. (At Stratford.) Slight showers in the evening. 2. Very cloudy morning. 5. Heavy rain, after 6 p. m. Lunar halo. 6. Rainy morning: high wind. 7. Still much wind: showery. 9. Very fine moonlight night. 12. Abundance of *Cirrocumulus*, gradually lowering, and arranged in close lines from S. E. to N. W. 14. A shower in the evening. 16. About five p. m. a *solar halo*, of short continuance: the sky at sun-set was (as usual of late) much coloured: there was a considerable diffused redness above the twilight, and some portions of the clouds, seen against this, varied from the usual indigo colour to a pale olive green: an indistinct appearance of *Nimbus* in the E. horizon.

RESULTS.

Prevailing winds, Northerly, with an interruption of some days continuance from the southward, producing for the time a considerable depression of the barometer, together with elevation of the mean temperature and rain.

Barometer: greatest height ....30·29 inches;
Least ....29·25 inches;
Mean of the period........30·009 inches.
Thermometer: greatest height..........75°
Least ....40°
Mean of the period....58·44°
Evaporation, 2·63 inches. Rain, 1·23 inches.

⁂ The observations from the 30th of the eighth month to the close of the period are chiefly those of my friend John Gibson, at the Laboratory, Stratford.

Tottenham, *Ninth Month*, 18, 1813. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

NOVEMBER, 1813.

ARTICLE I.

Biographical Account of Sir Isaac Newton.

(*Continued from p.* 247.)

AT the University of Cambridge, he spent the greatest part of his time in his closet; and when he was tired with the severer studies of philosophy, his relief and amusement was going to some other study, as history, chronology, divinity, chemistry; all of which he examined with the greatest attention, as appears by the many papers which he left behind him on those subjects. After his coming to London, all the time that he had to spare from his business, and from the civilities of life, in which he was scrupulously exact and complaisant, was employed in the same way: he was hardly ever alone without a pen in his hand, and a book before him; and in all the studies which he undertook, he had a perseverance and patience equal to his sagacity and invention. His niece, afterwards married to Mr. Conduitt, who succeeded him as Master of the Mint, lived with him about twenty years, during his residence in London. He always lived in a very handsome, generous manner, though without ostentation or vanity; always hospitable, and, upon proper occasions, he gave splendid entertainments. He was generous and charitable without bounds; and he used to say, that they who gave away nothing till they died never gave. This, perhaps, was one reason why he never made a will. Scarcely any man of his circumstances ever gave away so much, during his own life time, in alms, in encouraging ingenuity and learning, and to his relations: nor, upon all occasions, showed a greater contempt of his own money, or a more scrupulous frugality of that which belonged to the public, or to any society he was entrusted for. He refused pensions and additional employments that were offered him; he was highly honoured and respected in all reigns, and under all administrations, even by those whom he opposed; for in every situation he showed an inflexible attachment to the cause of liberty, and to the constitution of Great Britain. George the Second, and Queen Caroline, showed him particular marks of their favour and esteem, and often conversed with him for hours together. Queen Caroline in particular, who was so great a patroness of learned men, used to take delight in his company, and was accustomed to congratulate herself that she lived in the same country, and at the same time, with so illustrious a person.

Notwithstanding the extraordinary honours that were paid him, he had so humble an opinion of himself, that he had no relish for the applause which he received. He was so little vain and desirous of glory from any of his works, that he would have let others run away with the glory of those inventions which have done so much honour to human nature, if his friends and countrymen had not been more jealous than he was of his own glory, and the honour of his country. He was exceedingly courteous and affable, even to the lowest, and never despised any man for want of capacity; but always expressed freely his resentment against any immorality or impiety. He not only showed a great and constant regard to religion in general, as well by an exemplary life as in all his writings, but was also a firm believer of revealed religion; as appears by the many papers which he left behind him on the subject.* But his notion of the Christian Religion was not founded on a narrow bottom, nor his charity and morality so scanty, as to show a coldness to those who thought otherwise than he did in matters indifferent; much less to admit of persecution, of which he always expressed the strongest abhorrence and detestation. He had such a mildness of temper, that a melancholy story would often draw tears from him, and he was exceedingly shocked at any act of cruelty to man or beast; mercy to both being the topic that he loved to dwell upon. An innate modesty and simplicity showed itself in all his actions and expressions. His whole life was one continued series of labour, patience, charity, generosity, temperance, piety, goodness, and all other virtues, without a mixture of any known vice whatsoever.

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* I have heard it affirmed by some of the self-constituted philosophers of the present day, that Sir Isaac Newton believed the Christian Religion merely because he was born in a Christian country; that he never examined it; and that he left behind him a cart-load of papers on religious subjects, which Dr. Horsley examined, and declared unfit for publication. These Gentlemen do not perceive that their assertions are inconsistent with each other. Nobody who has ever read a page of Newton's works would believe that he could write a cart-load of papers on a subject which he never examined. Newton's religious opinions were not orthodox; for example, he did not believe in the Trinity. This gives us the reason why Horsley, the champion of the Trinity, found Newton's papers unfit for publication. But it is much to be regretted that they have never seen the light.

He was blessed with a very happy and vigorous constitution: he was of a middle stature, and rather plump in his latter years: he had a very lively and piercing eye, a comely and gracious aspect, and a fine head of hair, as white as silver, without any baldness. To the time of his last illness he had the bloom and colour of a young man. He never wore spectacles, nor lost more than one tooth till the day of his death. About five years before his death, he was troubled with an incontinence of urine, and sometimes with a stillicidium; both of which continued to afflict him, more or less, according to the motion to which he was exposed. On this account he sold his chariot, and went always in a chair; and he gave up dining abroad, or with much company at home. He ate little flesh, and lived chiefly upon broth, vegetables, and fruit, of which he always ate heartily. In August, 1724, he voided, without any pain, a stone, about the size of a pea, which came away in two pieces; one some days after the other. In January, 1725, he had a violent cough and inflammation of the lungs, upon which he was persuaded with considerable difficulty to take a house in Kensington, where he had, in his 84th year, a fit of the gout, for the second time, having had a slight attack of it some years before. This fit left him in better health than he had enjoyed for several years. In the winter of 1725, he wanted to resign his situation of Master of the Mint to his nephew, Mr. Conduitt: that Gentleman would not permit his resignation, but offered to conduct the whole business in his place; and, for about a year before his death, Sir Isaac hardly ever went to the Mint, trusting entirely to the management of his nephew.

On Tuesday, the last day of February, 1727, he went to town, in order to attend a meeting of the Royal Society. Next day Mr. Conduitt paid him a visit, and found him apparently in better health than he had enjoyed for several years. Sir Isaac was sensible of it himself, and told his nephew, smiling, that he had slept the Sunday before from eleven at night till eight in the morning, without waking. But his fatigue in attending the Society, and in making and receiving visits, brought his old complaint violently upon him. Dr. Mead and Mr. Cheselden were carried out to Kensington to see him, by Mr. Conduitt.

They immediately said, that his disease was the stone in the bladder; and gave no hopes of his recovery. The stone was probably removed from the place where it lay quiet, by the great motion and fatigue of his last journey to London. From this time he had violent fits of pain, with scarcely any intermission; and though the drops of sweat ran down his face with anguish, he never complained, nor cried out, nor showed the least sign of peevishness or impatience: and, during the short intervals from that violent torture, would smile and talk with his usual cheerfulness. On Wednesday, the 15th March, he was somewhat better, and fallacious hopes were entertained of his recovery. On Saturday, the 18th March, he read the newspapers, and held a pretty long conversation with Dr. Mead, and had all his senses perfect. But that evening, at six, and all Sunday, he was insensible, and died on Monday, between one and two o'clock in the morning. Thus, he reached the age of eighty-four years and a few months, and retained all his senses and faculties to the end of his life, strong, vigorous, and lively; and he continued writing and studying many hours every day till the period of his last illness. He died worth 32,000*l.* of personal estate; which was divided between his four nephews and nieces of half blood. The land which he had of his father and mother descended to his heir of the whole blood, John Newton, whose great grandfather was Sir Isaac's uncle. A little before his death he gave away an estate, which he had in Berkshire, to the sons and daughters of Mrs. Conduitt's brother; and an estate at Kensington to Mrs. Conduitt's daughter. From this lady the present Earl of Portsmouth is descended.

Sir Isaac Newton was buried with great magnificence, at the public expense. On the 28th of March, he lay in state in the Jerusalem Chamber, and was buried from thence in Westminster Abbey, near the entry into the choir. The spot is one of the most conspicuous in the Abbey, and had been previously refused to different Noblemen, who had applied for it. The pall was supported by the Lord High Chancellor, the Dukes of Montrose and Roxborough, and the Earls of Pembroke, Sussex, and Macclesfield, being Fellows of the Royal Society. The Hon. Sir Michael Newton, Knight of the Bath, was chief mourner, and was followed by some other relations, and some eminent persons intimately acquainted with Sir Isaac. The office was performed by the Bishop of Rochester, attended by the Prebend and Choir. A magnificent monument was erected to his memory, with the following inscription:—

H. S. E.
ISAACUS NEWTON, EQUES AURATUS,
QUI ANIMI VI PROPE DIVINA
PLANETARUM MOTUS, FIGURAS,
COMETARUM SEMITAS OCEANIQUE ÆSTUS,
SUA MATHESI FACEM PRÆFERENTE,
PRIMUS DEMONSTRAVIT.
RADIORUM LUCIS DISSIMILITUDINES,
COLORUMQUE INDE NASCENTIUM PROPRIETATES,
QUAS NEMO ANTEA VEL SUSPICATUS ERAT, PERVESTIGAVIT.
NATURÆ, ANTIQUITATIS, S. SCRIPTURÆ,
SEDULUS, SAGAX, FIDUS INTERPRES,
DEI OPT. MAX. MAJESTATEM PHILOSOPHIA ASSERUIT,
EVANGELII SIMPLICITATEM MORIBUS EXPRESSIT.
SIBI GRATULENTUR MORTALES, TALE TANTUMQUE EXTITISSE
HUMANI GENERIS DECUS.
NATUS XXV. DECEMB. MDCXLII. OBIIT XX. MART.
MDCCXXVI.

The mathematical discoveries of Sir Isaac Newton were so numerous and so important, that it is no easy task to give an idea of them. As his geometrical studies were conducted, in a great measure, without a master; and as the first books to which he paid particular attention were the Geometry of Descartes, and the Arithmetic of Infinites of Dr. Wallis, he never possessed any intimate acquaintance with the methods of the ancient mathematicians; a circumstance which, as we are informed by Dr. Pemberton, he afterwards regretted; but which, probably, contributed to render his invention so fertile and so happy. He made a great many discoveries while perusing the two works above-mentioned; and we have complete evidence that he was in possession of all his inventions before the age of 24. A complete collection of his works was published in 1779, by Dr. Horsley, in five quarto volumes, accompanied by a commentary, which, however, is any thing but complete. It is to be regretted that the mathematical world is yet destitute of a good commentary on the works of this consummate mathematician. Some of his books, indeed, have been fully commented on. Thus the Jesuits' copy of the Principia, if it has any fault, abounds too much with notes; and Stirling's Commentary on Newton's Treatise respecting Lines of the Third Order, is excellent. We have also a very elaborate Commentary on his Universal Arithmetic; and perhaps his Optics stands in need of no other commentary than the few optical discoveries which have been made since he wrote, and which enable us to rectify one or two of his opinions on that difficult subject.

Newton communicated many of his original discoveries to Dr. Barrow, at that time Professor of Mathematics at Cambridge;

and by him they were made known to various other British mathematicians. He likewise entered into a correspondence with Collins and Oldenburg, and by them was induced to write several long letters to Mr. Leibnitz, in which he gave an historical detail of the way that he was led to some of his most considerable discoveries. All these letters were afterwards published in the *Commercium Epistolicum.* The correspondence also between James Gregory and Collins, published in the same book, throws considerable light upon the order and time of Newton's mathematical discoveries. One of his first discoveries struck him while perusing Wallis's Arithmetic of Infinites, about the year 1663. Wallis had shown the method of finding the quadrature of all curves, the ordinates of which are expressed by $(1 - x^2)^m$, x being the abscissa, supposing m a whole number, either positive, or negative, or zero; and that when m was respectively 0, 1, 2, 3, 4, &c., the areas corresponding to the abscissa x were respectively x; $x - \frac{1}{3}x^3$; $x - \frac{2}{3}x^3 + \frac{1}{5}x^5$; $x - \frac{3}{3}x^3 + \frac{3}{5}x^5 - \frac{1}{7}x^7$; &c.; and he showed, that if a number could be interpolated between x and $x - \frac{1}{3}x^3$ in the second series, corresponding to the interpolation of $\frac{1}{2}$ in the first series between 0 and 1, that this number would represent the quadrature of the circle. But Wallis could not succeed in making this interpolation; it was left for one of the first steps of Newton, in his mathematical career. Newton arranged the terms of the second series given above, under each other in order, and examined them as follows:—

$$\begin{array}{l} x \\ x - \frac{1}{3}x^3 \\ x - \frac{2}{3}x^3 + \frac{1}{5}x^5 \\ x - \frac{3}{3}x^3 + \frac{3}{5}x^5 - \frac{1}{7}x^7 \\ x - \frac{4}{3}x^3 + \frac{6}{5}x^5 - \frac{4}{7}x^7 + \frac{1}{9}x^9 \end{array}$$

On considering this table, Newton observed, that the first terms are all x; that the signs are alternately positive and negative; that the powers of x increase by the odd numbers; that the co-efficient of the first term is 1; that the co-efficient of all the other terms are fractions; that the denominators of these fractions are always the indices of x, in the respective terms; that the numerators in the second terms are the ordinary numbers; in the third terms, the triangular numbers: in the fourth terms, the pyramidal numbers; &c. These observations made him master of the laws that regulated the whole of the series. Hence he concluded, that having to develope in general $(1 - x^2)^m$, the series of numerators for the respective fractions in the different terms must be 1; m; $\frac{m \,.\, m-1}{1 \,.\, 2}$; $\frac{m \,.\, m-1 \,.\, m-2}{1 \,.\, 2 \,.\, 3}$, &c., for these are the expressions which represent the natural, triangular, and pyramidal numbers. Now this will hold good whether m be a whole number or a fraction. In the case which occasioned the investigation, namely, $(1 - x^2)^{\frac{1}{2}}$; $m = \frac{1}{2}$, and, consequently, the numerators deduced from the preceding formulas are 1, $\frac{1}{2}$, $\frac{1}{8}$, $\frac{1}{16}$, $\frac{5}{128}$, &c. These, multiplied into the terms of the series, namely, $x - \frac{x^3}{3} + \frac{x^5}{5} - \frac{x^7}{7} + \frac{x^9}{9}$, &c., give us the following series: $x - \frac{x^3}{6} - \frac{1}{8.5}x^5 - \frac{1}{16.7}x^7 - \frac{5}{128.9}x^9$, &c., a series which obviously represents the area of the circular segment, corresponding to the abscissa x. This investigation led him likewise to the discovery of the binomial theorem, so celebrated in algebra, and of so much importance in an infinite number of investigations.

Newton had already made these discoveries, and many others, when the *Logarithmotechnia* of Mercator was published; which contains only a particular case of the theory just explained. But, from an excess of modesty and of diffidence, he made no attempt to publish his discoveries, expressing his conviction that mathematicians would discover them all before he was of an age sufficiently mature to appear, with propriety, before the mathematical world. But Dr. Barrow having contracted an acquaintance with him soon after, speedily understood his value, and exhorted him not to conceal so many treasures from men of science: he even prevailed upon him to allow him to transmit to some of his friends in London a paper containing a summary view of some of his discoveries. This paper was afterwards published under the title of *Analysis per Equationes Numero Terminorum Infinitas*. Besides the method of extracting the roots of all equations, and of reducing fractional and irrational expressions into infinite series, it contains the application of all these discoveries to the quadrature, and the rectification of curves; together with different series for the circle and hyperbola. He does not confine himself to geometrical curves, but gives some examples of the quadrature of mechanical curves. He speaks of a method of tangents, of which he was in possession, in which he was not stopped by surd quantities, and which applied equally well to mechanical and geometrical curves. Finally, we find in this extraordinary paper the method of *fluctions* and of *fluents*, explained and demonstrated with sufficient clearness; from which it follows, irresistibly, that before that period he was in possession of that admirable calculus: for the editors of this paper, which was published in the *Commercium Epistolicum*, attest that it was faithfully taken from the copy which Collins had transcribed, from the manuscript sent by Barrow. At the request of Dr. Barrow, he drew up a full account of this method, which was only described in the first tract with great conciseness. This new work he entitled *Methodus Fluxionum, et Serierum Infinitarum.* This last book he meant to publish at the end of an English Translation of the Algebra of Kinckuysens, which he had enriched with notes. But, in consequence of the disagreeable disputes into which he had been dragged, by his discoveries respecting the different refrangibility of the rays of light, he altered his intention, and the treatise, to the great injury of mathematics, and ultimately, likewise, to the diminution of his own peace, lay unpublished till after his death.

About the time that this paper of Newton's was sent to London, or about the year 1668, James Gregory published his *Exercitationes,* a book which contained several important facts connected with the discoveries which Newton had made. In particular there is a new demonstration of Mercator's Series for the Hyperbola. Collins communicated Newton's discoveries to various mathematicians, and among others to Gregory. He first sent him Newton's Series for the Circle, concerning the accuracy of which Gregory at first had his doubts; but he soon discovered his mistake, and by pondering over the subject for about a year, there appears sufficient evidence from his letters in the *Commercium Epistolicum,* that he divined Newton's method, and consequently had the merit of discovering the fluctionary calculus at least in part. But he declined publishing any thing on the subject, as he states in one of his letters, that he might not interfere with the rights of the original inventor.

(To be continued.)

Article II.

Observations on the Quantity of Carbonic Acid Gas emitted from the Lungs during Respiration, at different Times, and under different Circumstances. By Wm. Prout, M.D. Of the College of Physicians, &c.

(With two Plates.)

It was discovered by some of the earliest experimentalists on respiration, that the quantity of oxygen gas consumed, and of carbonic acid gas formed, during that act, varied very considerably in the same individual, under different states of the system. "The circumstance," says Dr. Bostock, "was first noticed by Dr. Crauford, and afterwards more fully investigated by M. Jurine, of Geneva, and M. Lavoisier, that the respiration o the same animal in different states of the system, and under th operation of different external circumstances, affects the air in very different degrees.—The circumstances which have been discovered to influence the chemical effects of respiration are, the temperature of the air respired, the degree of muscular exertion, the state of the digestive organs, and the condition of the system as affected by fever." Dr. B. continues: "It is highly probable that other circumstances will be discovered by multiplying and varying our experiments upon the living body," and informs us, that "these different affections of respiration will undergo a farther examination in the third part of his essay."* Now this, I believe, has never been published; whether, therefore, he, or any other person, has anticipitated me in what I am about to offer, I am unable to determine: if so, my experiments will at least have the effect of corroborating theirs; and if not, their interesting results may possibly induce some one to repeat them, and thus either confirm their accuracy, or point out their errors.

Mr. Brande, also, in a paper on respiration, after having noticed the above circumstances in a general way, says, "the proportion, however, (of carbonic acid) varies in the same individual during the 24 hours, for I *have found* the quantity of carbonic acid gas emitted from my own lungs to be *rather less in the morning than towards the evening*; but this also varies in different people."† It may also be mentioned, that Messrs. Allan and Pepys, in their excellent paper on this function, found the quantity formed by the same animal during sleep to be *less* than when waking.‡

Such then, with perhaps a few other general facts of a similar kind, constituted the whole of our knowledge (or rather of mine) respecting this most important part of the phenomena of respiration, and this, together with some other reasons which will be shortly known to the public, induced me to think of undertaking a set of experiments with the hope of throwing some light upon the subject, and, if possible, to find out the laws which it obeyed. With this view, having contrived a simple apparatus, by means of which I could easily, and with considerable accuracy, analyze the respired air, I put myself upon a sort of regimen, which consisted in keeping myself as nearly as possible in the same state in every respect, and thus commenced the arduous task. To this plan I adhered as nearly as circumstances would permit for upwards of three weeks, making the experiments every hour, and sometimes oftener, during the day, and occasionally during the night also. Now the results obtained from this great mass of evidence, amounting to many hundreds of experiments, were generally consistent, and may be comprised under the two following laws :—

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* Essay on Respiration, p. 78. † Nich. Journal, vol. xi.

‡ Phil. Trans. 1809. Nich. Jour. vol. xxv.

Law I.—The quantity of oxygen gas consumed, and consequently of carbonic acid gas formed, during respiration, is not uniformly the same during the 24 hours, but is always greater at one and the same part of the day than at any other, that is to say, its maximum occurs between 10 a. m. and 2 p. m., or generally between 11 a. m. and 1 p. m.; and its minimum commences about $8^h\ 30'$ p. m., and continues nearly uniform till about $3^h\ 30'$ a. m.

Law II.—Whenever the quantity of oxygen gas consumed, and consequently of carbonic acid gas formed, has been by any cause increased or raised above the natural standard of the period, it is subsequently as much decreased or depressed below that standard, and *vice versâ.*

Illustration of Law I.—This law is subject to some remarkable variations, though I have never met with an exception to it. In all my experiments there has been constantly a greater quantity of carbonic acid gas given off in the middle of the day than at any other period of it. From what Mr. Brande advanced in the paper above alluded to, I was indeed prepared to meet with the reverse of this; and for some time felt inclined to suspect the accuracy of my experiments, till, by varying them in almost every possible manner, and with the same results, I could no longer resist their united evidence.

Generally, the degree and order of these variations are the following. The quantity of carbonic acid gas, which has remained stationary during the night at 3·30 per cent.,[*] its minimum, about $3^h\ 30'$ a. m.,[†] suddenly begins to increase, at first slowly, and afterwards more rapidly, till about noon, when it is usually as high as 4·10 per cent., or its maximum; from this point, however, it almost immediately begins to sink, at first rather quickly, and then more slowly, till about $8^h\ 30'$ p. m.,[†] by which time it generally arrives again at its minimum, 3·30 per cent., when it remains stationary, as before observed, till the morning. Hence the quantity given off in the middle of the day, when it is at its maximum, exceeds that given off in the night, when it is at its minimum, by about $\frac{1}{5}$ of the whole. The mean quantity given off in the 24 hours is 3·45 per cent. (See Table I.)

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[*] By this is meant that for every 100 cubic inches of air inspired, 3·30 cubic inches of oxygen gas are consumed, and consequently of carbonic acid gas formed. The same is to be understood of all the subsequent numbers.

[†] That is, at *the beginning and end of twilight.* Many circumstances have occurred to induce me to believe that the presence and absence of the sun alone regulate these variations. Future observations, however, must decide this curious question.

Having spoken of the law as it obtains generally, we come now to consider its variations: these may be classed under the two following heads :—

1. Variations in the quantity of carbonic acid gas, as given off by *the same* individual on different days.

2. Variations in its quantity, as given off by *different* individuals.

The first of these variations I have observed in a greater or less degree, several times; indeed, no two days are exactly alike. But by far the most remarkable day that has come under my observation was the 22d of August, the maximum of which, exactly at noon, was as high as 4·90 per cent., and the minimum, at 8^h 20′ p. m., as high as 4·15 per cent. I had been up all the preceding night, for the first time; and hence was inclined, though contrary to my judgment, to attribute it to that circumstance; but on this being repeated, no such event followed, as I expected. What is remarkable, it had evidently begun to rise on the evening of the 21st; for at 9^h 30′ p. m. of that day, I found the quantity as high as 3·50 per cent., at least 0·2 per cent. higher than the usual minimum. From this point it continued to increase gradually till day-break, or a little before 3 a. m., when it suddenly rose up to 3·90 per cent. At 9^h 50′ a. m. it was 4·00 per cent. At 10^h 25′ a. m. 4·40 per cent., where it continued till after 11 a. m., when it suddenly rose up to 4·90 per cent., as before mentioned. From this point, however, it sunk rapidly; so that at 12^h 30′ p. m. it was only 4·30 per cent., and afterwards, at 2^h 30′ p. m., as low as 3·90 per cent., from whence it rose, apparently gradually, to 4·15 per cent., where, as before stated, it was at 8^h 20′ p. m. When this remarkable period ended I do not know; but the next day it was very little, if at all, above the usual standard. It is proper to observe, that during this period I was not aware of the least difference whatever in my health, or any other circumstance that could enable me to account for it. I fancied my respiration was more free, and that I felt lighter than usual; and this might be the case; but these were so trifling that I am confident they would have escaped my notice had it not been particularly directed to the subject. The barometer varied from 29·80 to 29·70 during the day, which was rather below* the mean of the preceding and subsequent periods. The mean of the thermometer's range was 61. The hygrometer made considerable advances during the day towards *damp*: the air was calm, and showers occasionally fell composed of very large drops, as occurs in thunder weather: in the evening there was a good deal of rain. I have reason to believe, therefore, that the atmosphere was in an highly electrical state. What is singular, a friend who spent the evening with me, and who at my desire made the experiment about 10 p. m., was found to emit about 4·90 per cent., whereas since that time we have once or twice repeated the experiment at about the same hour, and found it only 4·10 or 4·20 per cent., which appears to be his usual minimum. Hence it would seem that the circumstance was not peculiar to myself.

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* Generally speaking, I think I have found the quantity increase during the sinking of the barometer.

Two days after this remarkable *increase*, there occurred a no less remarkable *decrease*, or depression below the usual standard. The maximum took place earlier than ever I had seen it, that is, at 10h 30′ a. m., and was also less, amounting only to 3·70 per cent. The minimum also took place earlier, and was greater than usual,* that is, at 7h 45′ p. m., it was 3·40 per cent, where it continued the whole evening. During this period the barometer was about 30·20, the thermometer 60, and, as in the former instance, I was not aware of any circumstance whatever to which I could refer it. The following day was also generally below the standard, though not in the same remarkable degree. In the annexed diagram I have drawn these extremes of variation along with the mean, in order that they might be rendered more conspicuous by the contrast. See Plate XII.: where the upper line represents the greatest deviation above the mean observed; the lower line, the least deviation.

Besides these, there are some minor variations, which may be here mentioned. One of them is a slight depression occasionally between 6 and 8 a. m., which I have attributed to a less active state of the system, on account of abstinence.† This is very trifling, amounting only to 0·2 or 0·3 per cent., and sometimes is not at all perceptible. Another is, an extraordinary diminution of short continuance, immediately after the maximum, sometimes even below the usual minimum itself. This I have observed only when the maximum has been greater, and more abrupt in its ascent, than usual. It may, I presume, be accounted for on a principle of the second law to be hereafter mentioned. One example of this variation we have in the extraordinary instance of great increase above described. When the increase has been gradual, the decrease has been constantly the same.

2dly. The quantity of oxygen gas consumed, and consequently of carbonic acid gas formed, in a given time, is certainly very different in different individuals. Future investigations, however, must decide this important question more

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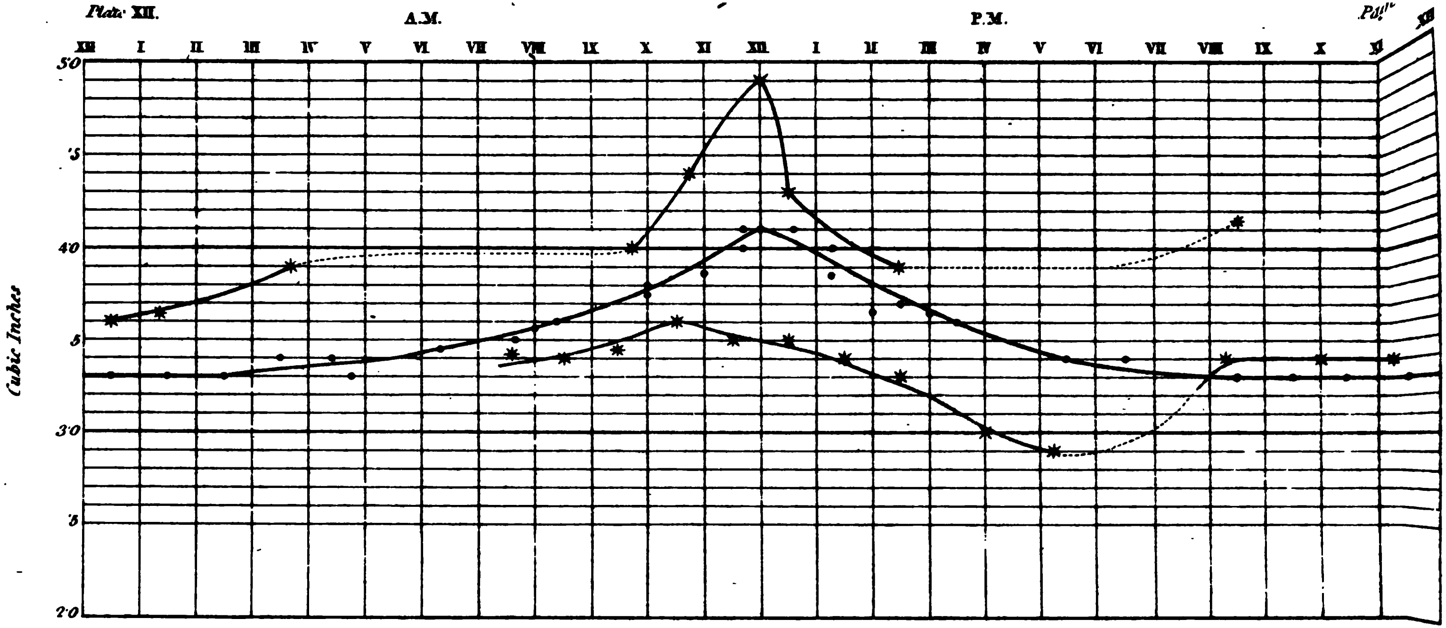
* I have generally found that when the maximum has been less than usual, the minimum has been greater in proportion: *i. e.* at least equal to the usual standard of 3·30 per cent., and sometimes above it. I have never seen the minimum permanently lower than 3·30 per cent.

† Perhaps, however, it may be accounted for on another principle. See Illustration of Law II.

Plate XII.



D^r. Prout on the Carbonic Acid Gas emitted from the Lungs during Respiration.

Engraved by W. Lowry for D^r. Thomsons Annals. Published by R. Baldwin Paternoster Row. Nov. 1.1813.

accurately than it can be done at present. I have collected all the evidence on this subject at present in my power; the results are certainly very discordant, and no doubt in some instances are to be attributed to errors. Most of the experiments were probably made, also, when the quantity was at its maximum; and this may account in part for the largeness of their results. This was undoubtedly the case with those of Messrs. Allan and Pepys, who inform us that all theirs were made "between breakfast and dinner."

	Cub. Inch.
M. Jurine,* of Geneva, "imagined" that for every 100 cubic inches of atmospheric air respired there were given off, of carbonic acid.	10
Goodwin† estimated the quantity at	10 or 11
Menzies,‡ from experiments made with considerable accuracy, at	5 or 5·1
Lavoisier and Seguin appear to have made it much less, especially in their later experiments. From the data in my possession I am unable to ascertain the precise proportion.	
Mr. Murray § found it vary from	6·2 to 6·5
Sir H. Davy ‖ from	3·95 to 4·5
Messrs. Allan and Pepys** from 3·50 to 9·50 per cent., according as the first or last products of an expiration were tried. They estimated the mean at about	8
Myself, from 4·10 maximum to 3·30 minimum. Mean of the 24 hours about......	3·45
A friend, from some partial experiments made in my presence, appears to emit, upon an average during the 24 hours, about	4·6

Of these my own is by far the least, so much so, that I should still almost suspect the accuracy of my experiments, had I not found the quantity emitted by my friend, under precisely the same circumstances, so much larger than by myself. Now we are both nearly of the same age, that is, about 30: of the two, he is rather my junior. With respect to habits, &c. I believe we are both equally regular, and enjoy equally good health. How, then, is this idiosyncrasy to be accounted for?

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* Ann. de Chim. tom. v. 261, &c. Bostock on Respiration, p. 85.

† Connection of Life with Respiration, p. 51.

‡ Menzies on Respiration, p. 50. Johnson's Animal Chemistry, vol. iii. p. 127.

§ System of Chemistry, vol. v. p. 493. Ed. 3.

‖ Researches, p. 431, &c. Bostock on Respiration, p. 84. Johnson's Animal Chemistry, vol. iii. p. 177.

** Phil. Trans. 1808, Part II. Phil. Mag. vol. xxxii. p. 242, &c.

TABLE I.

Showing the Mean Quantity of Oxygen Gas consumed, and of Carbonic Acid Gas formed, at every Hour of the Day and Night, as deduced from the 14th, 16th, and 25th of August, the Three most equal Days. Mean of the Barometer during this Period, 30·02; of the Thermometer, 62.

Hour.	Quan. of c. a. per cent.
A. M. 6	3·43
7	3·48
8	3·56
9	3·66
10	3·78
11	3·92
12	4·10
1	3·98
2	3·80
3	3·65
4	3·54
5	3·46

Hour.	Quan. of c. a. per cent.
P. M. 6	3·40
7	3·35
8	3·32
9	3·30
10	3·30
11	3·30
12	3·30
1	3·30
2	3·30
3	3·30
4	3·33
5	3·38
Mean	3·45 *

TABLE II.

Showing the Maximum and Minimum for Twelve Days.

Day. 1813.	Maximum.					Minimum.					Mean of 24 hours.
	Hour.	Quan. of c. a. per cent.	Pulse.	Bar.	Ther.	Hour.	Quan. of c. a. per cent.	Pulse.	Bar.	Ther.	
Aug. 12	1^{h} —′	4·10	75	30·08	66	8^{h} 15′	3·30	75	30·05	66	3·54
13	11 30	3·85	75	30·01	65	9 —	3·30	70	30·05	65	3·45
14	1 —	4·10	68	29·96	63	8 45	3·30	70	29·92	63	3·48
16	12 30	4·10	68	29·90	63	8 55	3·30	70	29·86	63	3·48
17	12 25	4·20	70	29·89	63	8 45	3·30	68	29·87	64	3·50
18	11 40	4·20	70	29·95	63	8 40	3·35	68	29·87	64	3·52
22	12 —	4·90	72	29·70	60	8 20	4·15	74	29·83	60	4·19
23	1 30	4·15	66	30·00	60	— —	—	—	—	—	—
24	10 25	3·70	74	30·20	58	7 45	3·40	70	30·23	60	3·32
25	11 25	4·10	68	30·20	59	9 —	3·40	68	30·20	60	3·42
26	11 25	3·75	70	30·20	60	8 —	3·30	68	30·18	60	3·40
27	11 40	4·00	70	30·13	59	9 30	3·30	72	30·18	60	—
Mean	12 2	4.10	70	30·02	62	8 38	3·40	70	30·02	62	3·53

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* This mean, as well as those in the following table, are obtained by first finding the mean of the quantity given out during the day, and adding it to the mean of that given out during the night, and then dividing the sum by 2.

Illustrations of Law II.—The circumstances which immediately *increase* the quantity of carbonic acid appear to be comparatively few, while those which *decrease* it are very numerous. The effects of the former class, also, are very inconsiderable and transient; while those of the latter, on the contrary, are much greater and more permanent.

The following are the chief circumstances which I have noticed to act in one or other of these ways; and, in conformity to the law, it will be found, by recurring to the list of experiments at the end of this part of the essay, that their action having ceased the standard was sooner or later resumed.

Exercise.—The effects of this are very various, according to its nature and degree, and according also to the time of its duration. Moderate exercise, as walking, seems always at first to increase its quantity; but after having been continued for a certain time, it ceases to produce this effect; and if prolonged so as to induce fatigue, the quantity is diminished. On the contrary, violent exercise seems to lessen its quantity even from the first; or if it does increase it, the effects are very trifling and evanescent. After violent exercise the quantity is always very much lessened. To this head also may perhaps be referred the act of *speaking*. I have found after a long silence the effects of speaking to be, to produce a slight increase, which, however, was only momentary, though the speaking was still continued. The *exhilarating passions*, also, to which perhaps the above might be referred in some degree, seem to produce a similar effect. See Exper. 1, 2, 3, 4, and 5.

Food.—No doubt different kinds of food have very different effects. As my object, however, at present, has been chiefly to discover general laws, I have not paid so much attention to this part of the subject as it demands, and which I intend to do. During the period in which these experiments were making, as before observed, I kept myself as regular as possible, with respect to food, &c., and only partook of the most simple, in order that it might not interfere with their results. The effects observed from food, therefore, have not been remarkable, and apparently little more than to keep up the quantity to the standard, and sometimes to raise it a little above, certainly never to depress it below, unless I took some fermented liquor, and then it was always depressed, as will be seen immediately. Long abstinence and fatigue certainly lessen the quantity; the former, however, when not in the extreme, hardly so much as might be expected. Thus I have found the quantity very little depressed, after fasting 21 hours, below the usual standard, and what it probably would have been had I taken my breakfast as usual. See Exper. 6 and 7. *Alcohol*, and all liquors containing it, which I have tried, have been found to have the remarkable property of diminishing the quantity much more than any thing else that has been made the subject of experiment. This was so unexpected on my part that I was prepared to meet with the reverse. I was first led, however, to suspect the accuracy of my opinion by observing that when I took *porter* with my dinner the quantity was always much reduced below the standard, when the reverse was the case when I only took water. This induced me to make some experiments on the subject; and their results were such as fully to persuade me that *alcohol, in every state, and in every quantity, uniformly lessens, in a greater or less degree, the quantity of carbonic acid gas elicited, according to the quantity and circumstances under which it is taken.* When taken upon an empty stomach its effects are most remarkable: in this case they appear to take place, and the depression to be greatest, almost instantaneously: after a short time, however, the powers of the constitution appear to rally, and the quantity rapidly increases; then it sinks again, and afterwards slowly rises to the standard.* Upon a full stomach, as after dinner, the effects of vinous liquors are more slow, but no less sure and remarkable; I have even thought them more permanent; but this might arise from my having taken a larger quantity than I chose to do upon an empty stomach. As long as their effects are perceptible, so long is the quantity of carbonic acid gas emitted below the standard. With me these effects go off with frequent yawnings, and with a sensation as if I had just awaken from sleep. Under these circumstances I have found the quantity generally much above the standard; and hence it would seem that the system is then freeing itself of the retained carbon. Very similar to the effects of alcohol are those of *tea* when strong. I had suspected this from what I had noticed after breakfast; and with the view of ascertaining the fact, prepared a strong infusion, three or four ounces of which I took cold when the quantity was at its maximum: the result was a very considerable diminution, as will be seen by recurring to the experiment. This will probably account for its exhilarating and other effects.† See Exper. 8, 9, 10, 11, and 12.

Sleep.—Of course I have not had it in my power to ascertain the quantity of carbonic acid given off during sleep. I am, however, decidedly of the opinion of Messrs. Allan and Pepys, that it is low, perhaps somewhat lower than the usual minimum. An argument in favour of this opinion is, that immediately after waking, and during the act, as it were, of expergiscence, I have always found the quantity very considerable, even in some instances as high as the maximum. This, however, is only of short duration; in half an hour the quantity, as before observed, is rather below the standard.

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* I have generally observed this sort of *oscillation* when the quantity has been suddenly and greatly raised or depressed from any cause, and I have been ready to account for it by supposing that the sudden and great exertions of the animal powers required to counteract the effects of a poison, or other injurious cause operating, made them, as it were, *overact* themselves.

† I have not tried opium, but have no doubt that its effects are precisely similar to those of alcohol. I hope soon, however, to investigate the effects not only of this, but of some other articles of the materia medica and alimentaria.

The *depressing passions* of every kind, and in short whatever produces that peculiar sensation which induces yawning, sighing, or any other deep inspiration,* have evidently the effect of diminishing the quantity, these appearing in fact but to be so many involuntary acts by which, as before observed, the system gets rid of its retained carbon. Anxiety and solicitude have a similar effect. One day the quantity was rapidly approaching to its maximum when a friend came in, whom I was particularly anxious to show the fact: but the extraordinary solicitude I felt on the occasion entirely defeated my purpose, and the quantity was less than I had almost ever seen it. Had this occurred but once, I should not have noticed it; but something of the kind has happened to me frequently. I mention it also to convince those who may be inclined to repeat my experiments, of the absolute necessity there is of keeping the body and mind in a state of the greatest possible uniformity. If this be not attended to the results will be discordant and unsatisfactory, as they were for the first day or two with me; so much so, indeed, that I was almost ready to give up the matter in despair, and to imagine that there was nothing like uniformity in it. By degrees, however, the laws which I have attempted to establish began to develope themselves; and having discovered these, and moreover acquired the habit of respiring and conducting the experiments with uniformity, I obtained the results which are now laid before the public.

Such, then, are the chief observations which have occurred to me in this investigation. The application of them to the establishment of the laws I have laid down are, I flatter myself, rendered so easy by the accompanying tables and experiments, that I deem it unnecessary to trouble my readers with it.†

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* Thus we yawn when we are sleepy to counteract the effects of that approaching state of the system which favours the retention of carbon and induces sleep. We yawn and stretch ourselves in the morning after waking to dispose of the carbon which had been retained during that act. So also yawning when we are hungry, and sighing when we are in distress, may be considered as extraordinary acts, by which that carbon is expelled which the diminished energy of the system in these states is unable to throw off by common means, but which, at the same time, its welfare demands.

† It may be proper to observe, that "the inspirations and expirations were somewhat deeper than natural, though not a great deal. The former were always made through the nose, and the latter into the bladder, till the stated number was completed. The results obtained are evidently to be un-

EXPER. I.

Aug. 14, 1813.—*Bar.* 29·94, *Ther.* 63.

Hour.	P. M	Standard quant. of carb. acid per cent	Observed ditto.	Pulse.	OBSERVATIONS.
5h	—'	3·46*	3·45	68	Before walk.
6	50	3·36	3·60	74	After a short ditto.
7	15	3·35	3·00	74	Example of oscillation alluded to p. 331, note.
8	—	3·32	3·20	70	
8	45	3·30	3·30	70	Return to standard.

EXPER. II.

Aug. 18.—*Bar.* 29·9, *Ther.* 64.

4	55	3·50	3·50	68	Before walk.
6	55	3·42	3·60	74	After a short ditto.
7	25	3·40	3·20	68	Example of oscillation above-mentioned.
7	50	3·38	3·30	70	
8	40	3·35	3·35	68	Return to standard.

EXPER. III.

Aug. 17.—*Bar.* 29·89, *Ther.* 64.

7	55	3·35	3·40	70	Before walk.
8	45	3·30	3·30	68	Five minutes after a very short and slow ditto;
9	30	3·30	3·30	64	This shows that moderate exercise scarcely affects the quantity.

EXPER. IV.

Aug. 19.—*Bar.* 30·17, *Ther.* 62.

12	37	4·00	3·70	115	Immediately after a very quick walk of ½ hour: great heat, and profuse perspiration.
12	55	3·92	3·70	96	
1	23	3·83	3·20	76	Example of oscillation above-mentioned.
2	23	3·70	3·00	68	
3	55	3·50	3·50	72	

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derstood as measures of the *capability* of the organs of respiration to form carbonic acid at any time, and not as measures of the *quantity* of it *formed* in a *given time*. The experiments of Messrs. Allan and Pepys show, that how frequently soever air is received into the lungs, it always returns charged with nearly the same quantity of carbonic acid. Hence during exercise, &c. when the circulation is quickened, respiration is so also, and a greater quantity of carbonic acid is formed in a given time than when these functions are in their natural state, though the quantity formed at a single inspiration may be less. Were not the respiration quickened when the circulation is so, a great portion of the blood would pass through the lungs without undergoing that change, so essential to life, which there takes place."

* Of course most of the numbers in this column are only the result of estimation. They are introduced merely to show what the quantity *would have been* according to the general law, had not the experiments been made.

EXPER. V.

Sept. 1.—*Bar.* 29·9, *Ther.* 60.

Hour.	P. M.	Standard quant. of carb. acid per cent.	Observed ditto.	Pulse.	OBSERVATIONS.
11h	—′	4·10	4·10	78	Before walk.
12	45	4·00	3·20	112	Immediately after rather a quick ditto of 1½ hour: fatigue.
1	10	3·90	3·20	84	Here, on account of the depression having been induced more slowly, there was no oscillation; but the quantity gradually rose up to the standard.
1	40	3·80	3·35	76	
2	—	3·72	3·35	76	

EXPER. VI.

Aug. 18.—*Bar.* 29·95, *Ther.* 64.

1	15	3·98	3·85	68	Before dinner.
2	25	3·82	3·55	70	Twenty minutes after ditto, with porter.
3	10	3·72	3·60	70	
3	55	3·64	3·60	68	
4	55	3·55	3·60	68	Standard resumed.

EXPER. VII.

Aug. 26.—*Bar.* 30·18, *Ther.* 61.

1	—	3·60	3·50	64	Before dinner. No breakfast. Little food taken every hour.
3	25	3·50	3·60	72	Twenty minutes after dinner. No porter, wine, &c.
4	30	3·45	3·70	68	This shows a slight increase after food.
5	25	3·40	3·40	64	Standard resumed.

EXPER. VIII. and IX.

Aug. 27.—*Bar.* 30·13, *Ther.* 60.

11	40	4·00	4·00	70	Before taking wine,
12	10	4·05	3·00	72	Five minutes after taking about three ounces.
12	30	4·00	3·40	76	Example of oscillation.
1	—	3·95	3·10	78	
1	25	3·86	3·10	78	
3	—	3·66	3·00	84	Twenty minutes after a walk and dinner.
3	30	3·60	3·10	78	
3	55	3·56	3·00	74	Ten minutes after taking at once ½ pint wine.*
4	5	3·54	3·00	78	Slight effects of ditto perceptible.
4	30	3·50	2·70	78	Very strong effects of ditto, as vertigo, &c. This is the lowest point to which I have ever seen the quantity reduced.
5	—	3·46	2·90	76	
8	5	3·32	3·60	76	Here the effects were gone off. Frequent yawnings, and a sensation of having been just awoke from a deep sleep.
8	45	3·30	3·50	76	
9	30	3·30	3·30	72	Standard resumed 5h 45′ after having taken the wine.

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* It is to be observed, that although I do not absolutely refrain from fermented liquors, yet that the quantity I am in the habit of taking is very small.

EXPER. X.

Aug. 31.—*Bar.* 30·15, *Ther.* 60.

Hour.	P. M.	Standard quant. of carb. acid per cent.	Observed ditto.	Pulse.	OBSERVATIONS.
12^{h}	—'	3·90	3·90	78	
12	30	3·87	3·60	74	Five min. after taking ʒiss of diluted alcohol.
12	50	3·82	3·45	78	Five min. after taking ʒii more ditto.
1	20	3·76	3·40	78	Five min. after taking ʒii more.
1	45	3·69	3·40	78	This shows that even in small and divided doses, which have been supposed to ensure the stimulant effects of alcohol, it still acts by diminishing the quantity of carbonic acid.
2	15	3·62	3·35	78	
2	45	3·56	3·30	76	

EXPER. XI.

Sept. 1.—*Bar.* 29·85, *Ther.* 61.

2	45	3·65	3·40	80	Twenty minutes after dinner and ½ pint wine. Effects less than I had before experienced after this quantity.
3	5	3·62	3·10	78	
3	35	3·58	3·30	76	Example of oscillation. Perhaps also of effects of food.
4	5	3·53	3·60	74	
4	35	3·49	3 65	74	
5	5	3·45	3·45	74	Standard resumed.

EXPER. XII.

Aug. 29.—*Bar.* 30·1, *Ther.* 60.

12	5	4·00	4·00	72	
12	20	3·97	3·90	72	Five min. after taking ʒiii of very strong tea, cold.
12	40	3·93	3·10	72	
1	5	3·88	3·50	72	

I shall close this long essay with a few general observations, and an account of the instrument employed in the experiments.

Of the causes operating to produce the above variations, no doubt the state of the circulation is to be considered as one. It is impossible but to suppose that when, *cæteris paribus*, a greater quantity of blood is exposed in a given time to the action of the air, that a greater quantity of carbonic acid gas must be formed, supposing this, at least, to be a common chemical process. It must, I think, be admitted, therefore, that this cause contri-

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This will account for the effects produced by what would scarcely act at all on one in the habit of taking larger quantities. Perhaps in these cases a bottle or two would be necessary to produce similar effects.

butes in some degree to the production of the phenomena in question, though, there is every reason to believe, in a very limited one. A very superficial examination of the above experiments will show that the quantity of carbonic acid gas formed bears scarcely any proportion to the numerical state, at least, of the pulse. This, indeed, I admit to be a very imperfect criterion of the quantity of blood circulating through a part in a given time, unless its strength and degree of fulness be taken into the account; but it might be asked, has the pulse been noticed to be uniformly stronger and fuller in the middle of the day than at any other time? and is it not constantly so under the operation of hard exercise and the influence of wine?

Mr. Brodie, in his excellent dissertations on the effects of poisons, has apparently demonstrated that respiration is altogether dependant on the brain; since, when the operations of this organ were destroyed or cut off, it almost immediately ceased, while the heart, which he found much less under nervous influence, continued for some time to act, and circulate dark coloured blood. He found also that alcohol, the essential oil of bitter almonds, and some other poisons, act as such simply by destroying the functions of the brain apparently by sympathy, and not by absorption; and that under the operation of these, respiration soon begins to be ill performed, and if their quantity has been great, at last entirely ceases.* This, then, while it enables us to account for the effects of alcohol in our experiments, seems also at the same time to prove, in general, that another cause besides the mere state of the circulation is immediately and largely concerned in the production of the phenomena in question. I conclude, therefore, that the *nervous system*, acting partly through the medium of the blood, and immediately by its influence over the function of respiration, is the grand source to which we must refer them all.

The laws which we have attempted to establish are no other than modifications of that general principle which prevails over all living actions. A state of depression ever follows a state of excitement; and the greater that excitement has been, the greater is the consequent depression. On the other hand, an uniformity of action at any point of the scale within which it ranges, is no less incompatible with life, than a great and sudden deviation towards either extreme. In all living actions, therefore, states of comparative rest alternate with states of exertion; and these alternations are evidently connected with the presence or absence of the sun; for according as this "great source of life and heat" is present or absent, are organized beings, in general, either awake and active, or asleep and inactive. Now

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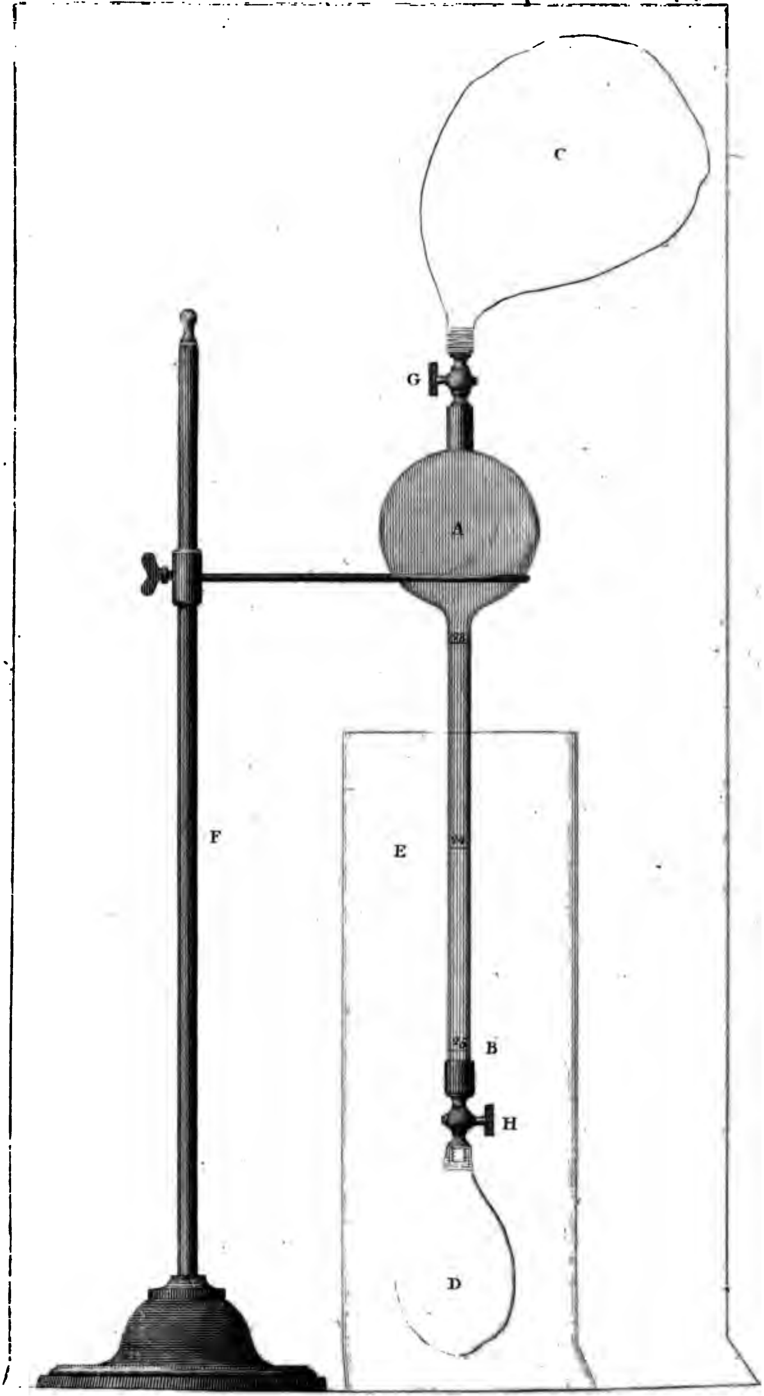
* Phil. Trans. 1811.



On the Carbonic Acid Gas respired from the Lungs.

Engraved by W. Lowry, for Dr. Thomson's Annals. Published by R. Baldwin Paternoster Row. Nov. 1. 18[illegible]

at noon this powerful agent of nature is at his height, and hence at this time may be supposed to exert his maximum energy; whatever, therefore, is under his immediate influence, must now be supposed to be affected most strongly; and if the hypothesis be correct,* that nervous action is so, it is perhaps easy to account for many important phenomena which it presents, and, among others, that of the greater elicitation † of carbonic acid at noon, respiration being considered as under its immediate influence.

Of the immediate operations, however, of the sun, or minor agencies under his direction, upon the nervous system, we know nothing, as every thing connected with this part of our frames is buried in the most profound obscurity. To refer, therefore, an animal function or operation to this system, is in other words to confess our ignorance of its nature; and I consider it better to do this than to run the chance of falling into error myself, and of misleading others by a demonstration that in the present state of our knowledge must be purely hypothetical. It is possible also that the laws I have attempted to establish may depend upon some idiosyncrasy, or suite of habits, peculiar to myself, and may not be general. If they do exist, no doubt they are liable to be modified in no small degree by habit, and even perhaps totally subverted. Others, however, must decide this. With respect to myself, I can only say that I have no doubt of their existence. At all events, speculations would be premature on a subject so slenderly founded as the *ipse dixit* of a single individual; and hence I shall defer my opinion till I have searched into the matter further myself, and heard the observations of others. Should they be established, I flatter myself they will throw no inconsiderable light on some important points in physiology and pathology.

The instrument with which these experiments were made is extremely simple, and the mode of using it may perhaps be learned by a mere reference to the figure [Plate XIII]. Its whole capacity to B is exactly 25 cubic inches of water at 60; of these 2 cubic inches are occupied by the neck, which is accurately graduated into tenths of a cubic inch. When used, it is filled with water, and a bladder, C, containing the respired air is screwed on, as represented in the figure; it is then fixed in the stand; and by turning the stop-cocks, G and H, the water is permitted to run out

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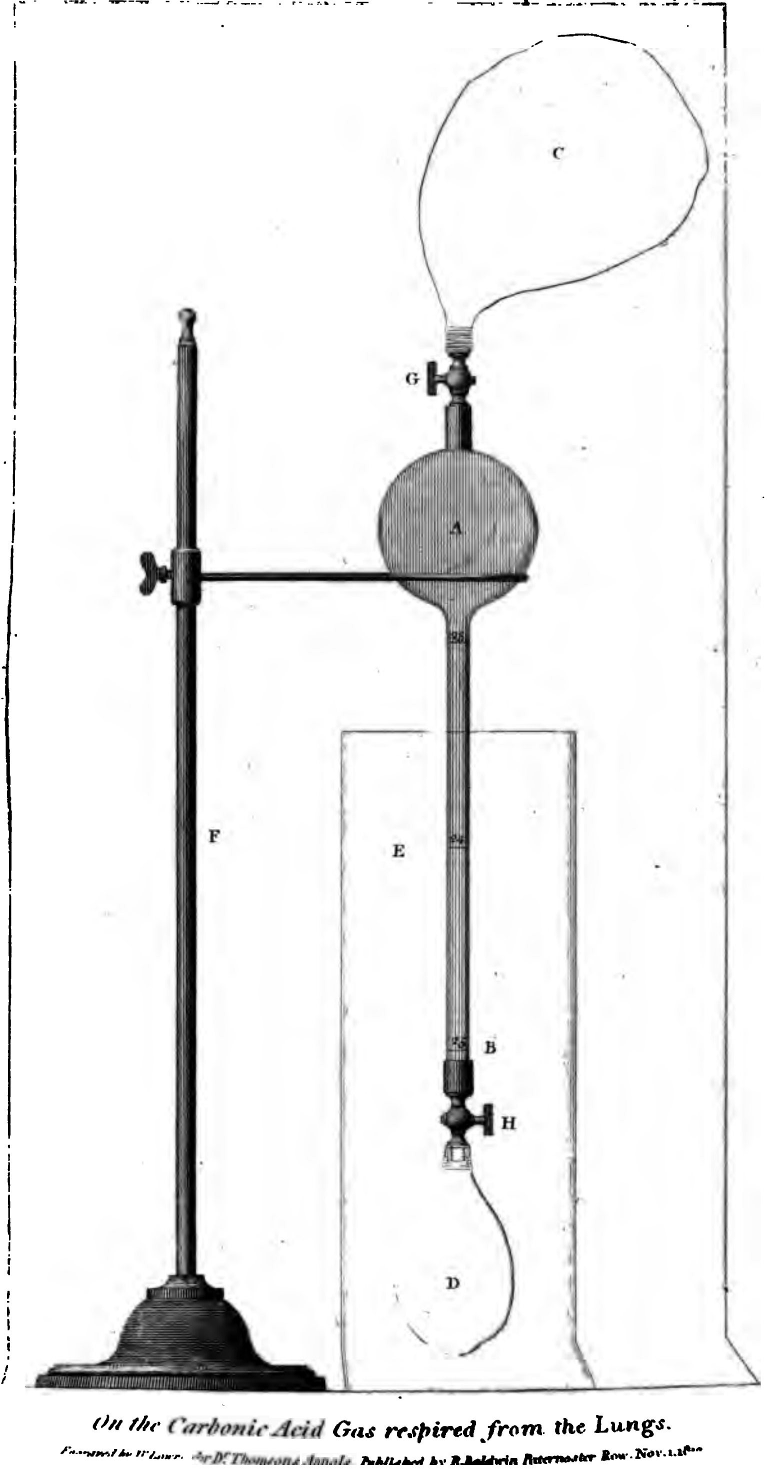
* Are not all the most powerful agents in nature, as heat, light, electricity, magnetism, &c. which appear to influence so immediately and energetically animal life, directed and governed, with respect to their periodical changes, by the earth's motions on its axis and in its orbit, which act only by placing it in different positions with respect to the sun?

† Not its *formation*: this is probably only a common chemical process. The regulation of the degree, &c. of its formation is to be understood,



On the Carbonic Acid Gas respired from the Lungs.

Engraved by W. Lowry, for Dr. Thomson's Annals. Published by R. Baldwin Paternoster Row. Nov. 1. 18

to B. After having stood some time, till it has acquired the temperature of the surrounding air, the stop-cock, G, is turned, and the elastic gum-bottle, D, containing a pretty strong solution of caustic potash, is screwed on at the bottom, as represented, in such a manner that no air be suffered to exist in it. The cock, H, is then turned, and the caustic potash introduced into the instrument, and well shaken, till all the carbonic acid is supposed to be absorbed; it is then returned into the bottle, and the stop-cock, H, secured. The instrument is then transferred to the stand, F, and the cock, H, again opened under water in the vessel, E, which rising shows the quantity of carbonic acid contained in the analised air, care being taken to raise or depress the instrument till the water stands exactly at the same height on the inside as the outside of the stem.

The bladder used on this occasion contained about 300 cubic inches, and the number of expirations was always six, which were made as nearly as possible in an uniform manner, both with respect to time and depth.

From some experiments made with mercury, and also expressly with the view of ascertaining the quantity of carbonic acid absorbed by the water used in the experiment, I have reason to conclude that the quantity, upon an average, amounted to about one per cent. This quantity, accordingly, has been added to all the numbers originally found by the instrument. As this source of error was general and uniform, it is obvious that the results were not comparatively affected by it. Other sources of error, which I am well aware are very numerous, were carefully guarded against by every possible attention to those circumstances in which they were most likely to take place.

Article III.

On the Method of separating Iron from Manganese. By Charles Hatchett, Esq. F.R.S. &c.

(To Dr. Thomson.)

DEAR SIR, *Mount Clare, Roehampton, Sept. 25, 1813.*

In the last Number of your *Annals of Philosophy* I have remarked a paper entitled "Contributions towards a Chemical Knowledge of Manganese, by Dr. John," first published in Gehlen's Journal für die Chimie, &c. in which the author states Gehlen's method of separating iron from manganese by succinic acid, and also his own process performed by means of oxalic acid. I am therefore induced to communicate to you a very simple and easy method of perfectly separating manganese from iron, &c. which I have for many years employed in various experiments and analyses, although I have not hitherto had occasion to state it in any publication.

Sect. I.

A solution of any ore of manganese having been made as usual in muriatic acid, and filtrated, must be diluted with three or four pints of cold distilled water. To this diluted solution add gradually pure ammonia, occasionally stirring the liquor until the acid has become perfectly neutralized; a few drops of ammonia may then be added; so that the liquor shall very slightly restore the blue colour to litmus paper which has been reddened by acetous acid.

The ferruginous precipitate must then be separated by filtration, and the liquor which passes will be found devoid of colour, and contains the pure manganese in permanent solution. It affords a white precipitate with prussiate of potash, and the oxide of manganese may be obtained by evaporating the solution to dryness, and by expelling the muriate of ammonia by heat; after which, if any of the muriate should be suspected to remain, it may be separated by washing the oxide upon a filter.

Sect. II.

Pure oxide of manganese may be also obtained by adding ammonia in considerable excess to the cold diluted muriatic solution, which then, without loss of time, must be poured upon a filtre of one fold. The liquor which passes becomes in a few minutes turbid and brownish, a pellicle is formed, and in about 24 hours the greater part of the manganese separates spontaneously in the state of brown oxide; and if the remaining liquor be evaporated to dryness and heated, the whole of the oxide will be obtained. But the objection to this method is, that the manganese is so rapidly separated from the ammoniacal solution, that it is scarcely possible, even by the quickest filtration, to prevent some part from being deposited on the filter, so that it becomes again mixed with the precipitate of iron, alumine, &c.

The effects of ammonia on the green oxide of iron are well known; but I do not recollect any instance of this oxide being found conjoined with manganese; and therefore it is not likely to interfere with the process above described.

When ammonia in great excess is added to the neutralized solution (Sect. I.), the same effect is produced as in Section II., and the manganese is spontaneously deposited.

Exposure to air does not appear to be necessary, for the mixture of the neutralized liquor (Sect. I.) and ammonia becomes turbid in close vessels.

If nitric acid be added to the neutralized liquor (Sect. I.) previous to its being supersaturated with ammonia, the manganese is not more speedily separated, and in some experiments the nitric acid seemed rather to retard that effect.

I need scarcely mention, that in cases of analyses, if any alumina, or calcareous earth, &c. should be suspected to have become mixed with the precipitated oxide of manganese, they may be readily separated by digestion in very dilute nitric acid.

If a mixture of the neutralized solution of manganese and of green muriate of copper be supersaturated with ammonia, as in Sect. II., the manganese separates, as already described, the copper remaining in solution; and if the liquor be evaporated to dryness, the copper may be taken up and separated from the oxide of manganese by digesting the residuum in warm ammonia. I am, dear Sir,

Yours very sincerely,

Charles Hatchett.

Article IV.

Mineralogical Observations. By Robert Jameson, Esq. F.R.S.E. Regius Professor of Natural History in the University of Edinburgh.

The mineralogical investigations I have been engaged in for some years past have enabled me to make several interesting observations. The following short enumeration contains a few of the most important of these:—

1. That primitive rocks contain no mechanical deposites, the conglomerated rocks in gneiss and porphyry being entirely of a chemical nature.

2. That greywacke is always a pure chemical deposite, and contains no mechanical intermixture.

3. That felspar occurs more abundantly in nature than is generally supposed, this mineral being one of the principal constituent parts of granite, gneiss, *clay slate*, porphyry, sienite, *serpentine*, *flinty slate*, *hornstone*, *greywacke*, *transition slate*, *striped jasper*, *oldest conglomerates*, besides forming in some degree the basis of most of the primitive, transition, and floetz trap rocks.

4. That the vast beds of conglomerate which rest upon, and

sometimes probably alternate with, transition rocks, are also chemical deposites.

5. That sandstone in many instances appears to be a chemical deposite.

6. That many of the fragments and fragmented appearances which occur in porphyry, limestone, and trap rocks, are of a chemical nature, and of cotemporaneous formation with the rocks in which they are contained.

7. That true primitive veins, those confined to primitive rocks, whatever may be their nature or magnitude, are often of cotemporaneous formation with the rocks they traverse.

8. That many of the veins in transition and floetz countries, even those extending for many hundred yards, and of great width, are of cotemporaneous formation with the rocks in which they are contained.

9. That strata of crystallized rocks may appear to run beneath an older rock when they really rest upon it, and have been formed after it.

10. That the various wavings in the strata of gneiss, mica slate, clay slate, greywacke, transition slate, and sandstone, are the effects of crystallization.

11. That the general, physical, and geographical distribution of petrifactions in the crust of the earth does not correspond with that of the present existing races of animals and plants.

ARTICLE V.

Some Mineralogical Observations on Cornwall. By Thomas Thomson, M.D. F.R.S.

(*Continued from p.* 253.)

In the rapid sketch of the south of Cornwall, which was printed in the last Number of the *Annals of Philosophy*, I have described nearly every part of the county that appears demonstrably to belong to the *transition class* of rocks. I suspect, indeed, that this class of rocks extends considerably farther north than I have been able to trace it: nor would it be surprising if the whole of the county should be hereafter proved to be transition; but as I observed no facts to support this opinion, we cannot at present adopt it.

The road from Merazion to Penzance winds along the sea-shore, and no rocks are to be seen till you reach the north end of the latter town. Here a bed of blue stone is perceived shelving down towards the sea. This rock is thick slaty, and has very much the appearance of clay slate, but is considerably harder than any clay slate which I ever saw. It is dark blue, gives a white streak, and has little or no lustre. It will be considered, I presume, as a variety of clay slate; at least I do not know any other rock to which it can be so well referred.

The pier of Penzance is situated near the south end of the town. It is built of granite, and is just finished. Beds of a granitic rock are seen rising out of the sea just behind this pier. It is a remarkably hard rock, of a brownish grey colour, and consists chiefly of felspar, in which crystals of transparent quartz are imbedded, which give it the appearance of a porphyry. Mica is very sparingly distributed through this rock; you may examine different specimens before you perceive any. Over this granitic rock lies a bed of clay slate. It is thick slaty, like the bed at the north end of the town; but its texture is coarser, it is not so hard, and has a purplish colour.

These two clay slate rocks were the only ones of the kind that I observed in the whole peninsula, upon the east side of which Penzance is situated. They have none of the peculiar characters of primitive clay slate, but approach much more nearly to those of transition slate. Hence I am disposed to consider them as transition. They lie immediately over the granite rock, which, as far as I was able to determine, seems clearly to constitute the fundamental rock of this peninsula. Between Penzance and the Land's End, a distance of ten miles, no other stone but granite was to be seen. Granite blocks were thick scattered over the fields, they constituted the material of which all the stone hedges are constructed, and I met here and there with rocks of granite quarried, in order to procure materials for mending the roads. This granite is porphyritic, the felspar is brown, the quartz white, and the mica black. It contains numerous large crystals of white felspar, three or four inches long, and often two or three inches broad. These crystals give the stone a peculiar and rather beautiful appearance. It is common to observe black irregular shaped patches in this granite, which when seen at a distance resemble fragments of other rocks; but when examined more closely, they are found to consist of scales of black mica thick set in a base of quartz. I thought, at the time, that I perceived likewise some hornblende in this granite, and broke off some specimens on that account; but upon examining these specimens more carefully at Penzance, I found that the black matter was not hornblende, but mica. This kind of granite continues all the way to the Land's End. The rocks composing that promontory consist of it. They are perpendicular cliffs above 200 feet high; and the aspect which they have acquired, from long exposure to the weather, puts one in mind of the face of an old hay-stack partly cut away with a hay-knife. This brownish yellow colour is owing to the great preponderancy of felspar in this rock.

The Logan rock, about three miles south from the Land's End, consists of an immense block of the same kind of granite, so poized upon a point as to be moveable by the force of a man. Veins of schorl are said to exist near it, but I did not see them.

The primitive granite of Cornwall appears to constitute a tract of no great height nor breadth, extending from the Land's End to the extremity of Dartmoor in Devon, and therefore running in a direction not very far from south-west and north-east. I did not indeed trace it the whole of this way, but I crossed it in three several places, at a considerable distance from each other, and therefore have reason to think it highly probable that the rock continues all this way (a length of about 120 miles). The Scilly Islands are likewise granite, and are doubtless a continuation of the same range. This granite range terminates in the sea at both extremities. It rises gradually all the way from the Land's End to Dartmoor, where it constitutes mountains, some of which, as we learn from the trigonometrical survey, are 1549 feet above the level of the sea.*

This granite range of ground does not exceed three miles in breadth (and indeed at Redruth appears narrower) in Cornwall, but in Devonshire its breadth is considerably greater. About half way between Bodmin and Launceston, you cross another granite tract. It would seem that it branches off from the east and west granitic range nearly at right angles; or perhaps the granitic range becomes wider at this place than usual. It appeared to go north some miles beyond the high road from Bodmin to Launceston, and constituted all the way a tract much more elevated than the rest of the country, and rising in some places so as to form mountains.

Clay slate rests upon this granite range, both on the north and south side This clay slate on the south side dips south, and on the north side, north, showing evidently that it lies over the granite. On the west side of the north and south granite range it dips west; and at the east side, towards Launceston, it dips east. When we examine it at a considerable distance from the granite range, it appears horizontal. These observations, which I made in a variety of places, leave no doubt that this extensive bed of clay slate every where covers the granite, and is nowhere covered by it. It is likely that the granite rock extends very widely, and may even constitute the basis of the county;

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* According to that survey, Brown Willy is 1368 feet, and Rippintor 1549 feet, above the level of the sea. According to Dr. Berger, Cape Cornwall near the Land's End is 229 feet, and Carnbrae (a hill at Redruth) 697 feet, above the level of the sea.

for the mines are all situated in the clay slate, frequently at no very great distance from the granite. Now when these mines go deep, it sometimes happens that they penetrate through the whole of the clay slate, and continue on in the granite; thus affording a demonstration that the granite lies under the clay slate. This is the case, for example, in the mine of Huel Unity, two miles east from Redruth.

This clay slate, in every part of the county that I observed it, as for example at Truro, at Bodmin, at Launceston, and in the mines extending about ten miles east and west from Redruth, has the characters of transition clay slate. It has not the continuous lustre which distinguishes primitive slate; but is almost dull, glimmering only from the specks of mica which are scattered through it. The colour is most commonly dark blue, or purple. This rock is uniformly distinguished in Cornwall by the name of *killas*. Hence, if my opinion be well founded, the Cornish term killas is synonymous with transition slate. I have every reason to believe that it is never applied to *greywacke*. Hence the substitution of it for greywacke, as has been proposed by Mr. Playfair, and practised by Mr. Allan and Sir James Hall, would be the introduction of an error, and would uniformly mislead every mineralogist who drew his notions of greywacke from the killas of Cornwall.

In the parish of St. Stephen's, about five miles from St. Austle, there occurs in the granite range a curious rock, which is wrought in many places, and constitutes the material from which the porcelain clay for the Staffordshire potteries is obtained. This rock consists of a white and very soft felspar basis, which crumbles to powder between the fingers, and falls down in the state of clay in water. Small transparent quartz crystals are thick scattered through this basis. I could perceive no mica, and have reason to believe that the rock contains none. This rock is sent to Staffordshire in two states. It is quarried out in lumps, and sent in that state without any preparation. These lumps are ground down in Staffordshire, and the quartz powder acts the essential part of counteracting the too great tendency which the clay has to contract. The second mode in which it is sent to Staffordshire is quite different. The rock is exposed to a current of water, which, washing off the fine clay particles, runs milk white into pits prepared for the purpose. Here the water gradually evaporating leaves the fine white clay, which is dug out in squares, and when dry packed up in barrels, and sent to the sea-shore. Only the purest water can be used for this purpose, and great care is taken that the instruments used are of such a nature as not to communicate any metallic stain, which would render the clay unsaleable.

The excellence of this rock consists in the almost total absence of metal. It seems to be composed of nothing else than silica and alumina. I do not know, indeed, that it has ever been subjected to a chemical analysis; but this ought to be its composition, on the supposition that it consists of pure felspar and quartz, and that no metallic matter is present.

Almost all the mines in Cornwall are confined to a tract about six miles broad, running from the Land's End to the extremity of Dartmoor, seemingly in the same direction with the granite range, and just beside it. The ores are all in veins, the most important of which run in an east and west direction. It has been observed that the principal tin veins run from south-west to north-east, while the principal copper veins run from west to east. There are a considerable number of these veins not exactly parallel to each other, but approaching to that direction. In Dolcoath mine, for example, I counted eight veins. These veins in Cornwall are called *lodes*. During their course they vary considerably in thickness, and even in their direction. They go to a very considerable depth; and not one of them, as far as I could learn, has ever been mined to its extreme depth. The ores have generally been observed to increase in value as the vein deepens. One of the deepest mines in Cornwall is Dolcoath, about a mile south-west of Redruth. Its present depth is 220 fathoms. I believe Huel Unity was still deeper; but of late the deepest part of it has been abandoned. This was owing to a mine on the south side of it having been forsaken. The water, in consequence, accumulated to such a degree that the expense of drawing it up was greater than the mine could afford. This obliged them to forsake the lowest part altogether. It is common to find tin at the top of copper veins; but never copper at the top of tin veins. The upper part of the vein is called *gossan* by the miners. It contains less ore than the lower part, and the veinstones are always stained yellowish red, as if they had been steeped in ochre. The rock which borders on the vein on both sides is called the *country*. This, about Redruth, is always transition slate; except at a considerable depth. Thus at Huel Unity, at the depth of 50 fathoms, the country becomes *granite*. To this granite they give the name of *grauen*.

The veinstones are frequently *quartz*. Sometimes they consist of *hornstone porphyry*, a stone to which the miners give the name of *elvan*. I got specimens of *elvan* from the overseers of five or six different mines, and all of them consisted of hornstone porphyry, differing indeed somewhat in its colour, and in the size of the crystals which it contained; but in other respects the same. Hence I conceive there can be no doubt that *elvan* in Cornwall signifies *hornstone porphyry*. Sometimes clay occurs in the veins. To this the miners give the name of *fleuckan*.

The only copper ore of any consequence that occurs in these veins is *copper pyrites*. The arseniates of copper, &c. are too small in quantity to be of any importance in a mining point of view. Iron pyrites and arsenical pyrites are likewise very common attendants. They are both confounded under the name of *mundic*. Wolfram is rather scarce; for I did not observe any specimens of it in any of the mines that I visited. Fluor spar is a very common veinstone. It is always green, and is distinguished by the name of *green spar*. Galena occasionally occurs; sometimes, as in Huel Anne, in such quantities that it is collected and sold. In these cases its never-failing companion, *blende*, is also met with; and blende is likewise in some mines collected and sold. In the *united mines* on the south side of Huel Unity I observed a good many specimens of native copper among the ore. The *arseniate of lead*, a beautiful mineral lately analysed by Mr. Gregor, has only been found in Huel Unity mine, in a cross vein at some considerable depth. It was in the same mine, unless I am mistaken, that some of the finest specimens of the arseniate of copper occurred. Green mica, or oxide of uranium, has been found at Beeralston, at the eastern extremity of the county; and Mr. Gregor showed me a yellow variety of the same ore from a mine in the parish of St. Stevens. It was in the same mine that the sulphuret of tin formerly occurred.

Besides the veins running easterly and westerly, which are those of the most consequence, there are other veins which run from north to south, and on that account are called *cross courses*. These veins are usually filled with quartz. They sometimes contain ores, but most commonly not. They constitute the newest of the Cornish veins; for they always cut through the other veins, and frequently alter their positions; so that the portion of the vein on the west side of the cross course lies farther north than the portion of the same vein on the east side. These cross veins vary in breadth from a few inches to 20 fathoms. The greatest of them is about three miles east from Redruth, and has been traced from sea to sea. In some places it is 20 fathoms wide, in others only a few feet. It heaves all the veins which it crosses about 50 fathoms. That portion of the vein on its west side is 50 fathoms farther north than the part of the vein on the east side.

The tin ore is always *tinstone*, or oxide of tin. This ore extends the whole length of the mining district. The Scilly Islands contain the same veins, and formerly tin mines were wrought in them. No doubt this was the reason why they received from the ancients the name of Cassiterides, for κασσιτερος, as every body knows, is the Greek name for tin. During Queen Elizabeth's reign considerable quantities of tin were raised in Dartmoor. Hence there can be no doubt that the great tin veins extend over the whole length of Cornwall and Devon, and lose themselves at both extremities in the sea.

Tin has been raised in Cornwall from time immemorial. It is well known that the Phenicians traded to this county for tin at least 3000 years ago. Indeed, if we believe Camden, the name *Britain* was given to our island on account of the tin which it yielded; for it signifies, he says, *the land of tin.* Remains of the old tin mines are now and then found in Cornwall. They are never deep: and it would appear, from the way in which they are constructed, that the mode of raising the ore was to throw it, by means of shovels, to a recess cut in the shaft for that purpose; from this it was thrown to another recess; and this was continued till it reached the top of the mine. The copper mines constitute at present by far the most important mines in Cornwall. Prodigious quantities of copper are raised there annually. It is not much more than 100 years since the mining for copper commenced; and during the last 30 years it has been carried to a much greater extent than before.

As far as I had the means of observing, the Cornish mines are managed with very considerable skill. The machinery is all good, and in many places excellent. They are obliged almost every where to use steam-engines, on account of the scarcity of water. A regular monthly statement of the work done by these steam-engines is collected, a plan which has been attended with much good effect already. The best steam-engines, with the consumption of one bushel of coals, raise 27,000,000 of gallons of water one foot high. The little water which can be collected is applied to drive machinery with great skill; but the expense of procuring water is generally so great as to render steam-engines preferable.

If I were to presume to find fault with any part of the process followed by the Cornish miners, it would be the manner of washing the tin ore after roasting it. The tin ore is always mixed with copper pyrites, iron pyrites, and arsenical pyrites. To get rid of the arsenic (or *mundic*, as they term it,) this mixed ore is roasted in a reverberatory furnace. The powder after this process is washed in water; and by that means the tin ore, which is heavy, is easily separated from the copper ore, which is light. Now it appears to me that, by this roasting, a considerable portion of the copper pyrites must be changed into sulphate of copper. All this portion will be dissolved by the water used in washing, and entirely lost. I should propose to throw the roasted ore into a kind of pit, to cover it with water, stir it about, and after a few days draw off the water into a separate pit. Old iron thrown into this water would precipitate the copper: and I am persuaded that the quantity of copper thus saved would much more than pay the expense of the process. The ore, after the water is drawn off, may be washed in the manner at present practised.

The names by which the Cornish mines are distinguished are usually contrived by the first adventurers, and are often whimsical enough. The term *huel* (pronounced *will*), prefixed to every mine, is said in the Cornish language to signify *a mine*: the other name is taken from some accidental circumstance or other. Thus Dolkoath was the name of an old woman (Dorothy Koath) who lived upon the spot where the mine began working: Huel Alfred was named by Mr. Jenkins after his son Alfred Jenkins. *Huel Providence* was so called from the accidental way in which the vein was discovered.

I cannot terminate this sketch without noticing the roads in Devonshire and Cornwall. They are by far the worst which I have seen in any part of Great Britain, and are very disgraceful to both counties. The road from Dorchester to Exeter is, I presume, very old. The great object seems to have been to get upon every possible hill; and the country is so hilly that the whole road, with very little interval, consists in steep ascents and descents. Horses cannot possibly drag half a load upon it. No great skill would be necessary to construct a road between Dorchester and Exeter almost perfectly level. Why this is not done is not easy to conceive. The counties of Dorset and Devon would soon pay the expense in the difference of the loads which could be carried from place to place, and in the greater ease with which the level road could be kept in repair. These observations apply with double force to the road between Plymouth and Falmouth, many parts of which are as badly constructed as it is possible for them to be. In a country where the internal communication is so great as in England, it is amazing to see so long and important a line of road so much neglected.

Article VI.

An Account of the Explosion of Inflammable Air which very lately occurred in the Hall Pit at Fatfield. Drawn up for the "Annals of Philosophy."

I promised in my account of the accident which occurred in the Collingwood Main Colliery, which you have inserted in the last Number of your excellent Journal, that you should hear from me again upon the subject of explosions in coal-mines; but I did not expect that it would so soon have fallen to my lot to record an accident of greater importance, from the number of lives which were lost, than that of the Collingwood Main

Colliery. On Tuesday, the 28th of September, at half-past twelve p. m. the inflammable air of the Hall Pit Colliery at Fatfield, distant nine miles from Sunderland, exploded, by which *thirty-two* pitmen were instantly killed, and four wounded. The overman, Mr. Joseph Noble, reports to the writer of this article, " that the same morning on which this accident happened, he considered himself as safe in this colliery, as he was at that moment in his own house." At the same time it is proper to remark, that three explosions of inflammable air had taken place at different periods in this colliery, by which three men were killed each time.

This explosion is supposed to have been occasioned by the falling of a stone from the roof of the mine, which carried the inflammable air with it, so as to bring it into contact with the pitmen's candles. All the men who were in the workings were killed; and of the four young men who were at the shaft, one was killed upon the spot, and three escaped with life. George Noble, son to Mr. Noble the overman, informs me that he was one of the people employed at the shaft, and had the good fortune to escape, with the exception of a considerable swelling over the whole body, which continued for a few hours, and then gradually subsided. This swelling was supposed by his friends to be occasioned by the carbonic acid gas of the mine. This young man also mentions that he heard three distinct reports, which suddenly followed each other; and that he was rapidly driven to the distance of four yards by the violence of the explosion; and about this time he was deprived of sensation, for the first circumstance which he remembers was his difficulty of breathing, and at the same time he was seized with pains in different parts of the body. He is now (Sept. 30) perfectly recovered, though still very weakly. All the unfortunate men are now got out of the colliery, with the exception of an old man, David Redley, who held the appointment of deputy overman.

This coal-mine was understood to be as carefully worked, and as free from danger, as any mine in the kingdom: so much so, that many of the pitmen, who happened to be at their own houses at the time when the explosion happened in the Hall Pit Colliery, could not believe that such an accident had occurred; and all persons who knew the Hall Pit Colliery were impressed with the same favourable opinion. No blame whatever is attachable to any person concerned in this colliery, as far as I can ascertain. What person in his sober senses will believe that the present mode of management of coal-mines is the best that can be devised for the prevention of such accidents? Against such melancholy facts, what arguments can interested persons bring? How dreadful must be the situation of those honest and industrious people, who know not how soon they may be "gathered to their forefathers," by a sudden and violent death! How long are such frequent catastrophes to be permitted? The Slave Trade, when at its acme, did not carry such horror to the feeling mind as these shocking explosions.

Sept. 30, 1813. Φίλελεεινόςι

ARTICLE VII.

Experiments on the Stability of Vessels containing a Well on Board. By Col. Mark Beaufoy.

(To Dr. Thomson.)

MY DEAR SIR, *Hackney Wick, Oct.* 9, 1813.

HAVING often heard of the fishing-vessels employed in the North Seas being lost, and that no part of the vessels came on shore, it was concluded they must have foundered in the Ocean; and having paid some attention to naval architecture, I was led to consider, if the cause of the sinking of vessels occupied in the fisheries might not be partly attributed to the faulty construction of the wells in which the fish are preserved. As the surest road to come at facts is by the test of experiment, I have taken the liberty of troubling you with the result of my inquiries on this subject, which I hope may be useful in saving the lives and properties of our countrymen.

Having caused a hollow parallelopipedon to be made, which measured in length very nearly 24 inches, in breadth 9 inches, and in depth 7 inches; it was loaded until it was immersed in water 4½ inches, or exactly one-half of its breadth; and the ballast was so adjusted that the centre of gravity of the figure was precisely, in each experiment, 2¼ above the bottom, or in the same point at the centre of gravity of the displaced fluid. This concave parallelopipedon had likewise two partitions, distant from each other 13·57 inches, for the purpose of forming a well; this well had also a deck, the under side of which was even with the surrounding fluid; when the figure floated in an upright position, the breadth of the well was 8½, and the depth 4¼ inches.

By comparing these experiments together, it is evident that the first and second experiments very nearly coincide: the variation may justly be attributed to the unavoidable inaccuracy in making the experiments; and the conclusion to be drawn from the near coincidence is, that the water in the well, when confined, acts as ballast, as a lump of ice would, on the supposition that ice and water were of the same specific gravity.

Experiment the third, in which the holes of the well are opened, and the water, when the vessel is inclined, permitted to escape, gives a very different result. The vessel in this case becomes more and more deficient in stability as the inclination increases, because the water escapes to windward; which produces the same effect as if so much ballast was placed on the lee side: therefore, to insure the safety of the vessel, by preventing it from oversetting, the well-deck must be as much depressed as is equal to the greatest inclination the vessel may receive by the pressure of sail; but, as in many cases the capacity of the well would be too much diminished by placing the well-deck so low down, the escape of the water might be prevented by internal valves, which would close of their own accord by the action of the fluid when running out. These valves could with facility be kept open by a line, one end being fastened to the valve, and the other end coming on the deck; by this means the water contained in the well could be shifted for the preservation of the fish. The advantages of a well, it is conceived, might be usefully employed on many occasions: for instance, if a boat on shore is immediately wanted, and ballast requisite, it is evident it must be put on board either before the boat is launched, or afterwards; if before, it becomes heavy, and difficult to move; if after, time is lost; but not so with a well; for in this case time will be saved, as the boat will take in its ballast whilst under sail: in this case the aperture may be made through, or by the side of, the keel, (as it matters not in what part of the body the orifice is made,) and stopped with a plug, when the well is filled. This plug-hole through the well-deck will suffer the air to escape while the well is filling, and by its insertion both holes closed. As I am on this subject, a thought has struck me which may be deemed worthy of consideration by the Public Boards connected with the naval department; that if the orlop deck of a 74-gun ship were made water-tight, and if by accident the ship got aground, or by any other cause sprung a dangerous leak, I have no doubt if the hatches were battened down, and otherwise well secured, it would swim by this deck, and have considerable stability, by which the lives of the crew, and the materials of the ship, would be saved.

Experiment.

	Oz. Avoird.
Weight of the water displaced by the parallelopipedon was	559·06
Weight of the parallelopipedon substract	111·56
Remains the weight of ballast	447·50
When the well is filled must be deducted the weight of water	264·02
Remains the weight of ballast used in Experiments II. and III.	183·48

	EXPER. I.		EXPER. II.		EXPER. III.	
1	2	Difference.	2	Difference.	2	Difference.
5	2·4687	2·4844	2·3437	2·6638	2·3759	1·9375
10	4·9531	2·7812	5·0075	2·7248	4·3125	1·8487
15	7·7343	2·9814	7·7343	3·2062	6·1612	2·2138
20	10·7187	3·2500	10·9375	2·9375	8·3750	2·5156
25	13·9687	3·3750	13 8750	3·4687	10·8906	2·6406
30	17·3437		17·3437		13·5312	

The first column shows the degree of inclination produced by the weights (Column 2.) applied to a lever of 24·71 inches in length.

I remain, my dear Sir,
Your much obliged and faithful
MARK BEAUFOY.

ARTICLE VIII.

Experiments on the Nature of Azote, of Hydrogen, and of Ammonia, and upon the Degrees of Oxidation of which Azote is susceptible. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry and Pharmacy in Stockholm.

(Continued from p. 284.)

LET us now examine the composition of *nitrous acid.*

Gay-Lussac, in a memoir on this subject, has endeavoured to prove that this acid is formed when three volumes of nitrous gas are condensed by one volume of oxygen gas; that is to say, that one volume of azote requires $1\frac{2}{3}$ volume of oxygen to be converted into nitrous gas. According to the results of that celebrated chemist, the degrees of the oxidation of azote are 1, 2, $2\frac{1}{3}$, 4; but that series does not correspond with what we have found respecting other oxidable bodies. This consideration renders Gay-Lussac's determination of the composition of nitrous acid doubtful.

Mr. Dalton (*New System of Chemical Philosophy*, ii. 231,) determines the constituents of this acid to be 32·7 azote and 67·3 oxygen. This is a greater proportion of oxygen than Gay-Lussac obtained. According to the ingenious manner of Mr. Dalton of expressing chemical compounds, nitrous acid is composed of 2 atoms of azote and 3 of oxygen; but there is a strong objection against this hypothetical estimation. If we suppose a combination of 2 atoms of one body with 3 of another, we have equal reason to suppose the possibility of a combination of 3 atoms with 4, 5 with 6, &c. By thus increasing the number of atoms of each body united as much as the possibility of mutual contact will permit, we extend the bounds of chemical proportions indeed, but almost entirely destroy the doctrine of determinate proportions.

Among the experiments to determine the composition of nitrous acid, Mr. Dalton states, that the greatest quantity of nitrous gas, which one volume of oxygen gas is able to condense, amounts to 3·6 volumes. This determination deserves the more attention, because we shall find that it exhibits the real constitution of nitrous acid.

Sir Humphry Davy in his *Elements of Chemical Philosophy*, (vol. i. p. 265,) informs us, that, from experiments made on purpose, he considers nitrous acid as composed of 4 volumes of oxygen gas and 1 volume of azotic gas. According to this observation, the degrees of oxidation of azote will be 1, 2, 4, 5; but in this series we do not find the number 3, a combination which would certainly have appeared had the series been exact.

I published myself a set of experiments on the composition of nitrous acid; and as I set out from another point, I had different results from those which I have stated. I examined different combinations of nitrous acid with saline bases, and especially with the oxide of lead (*Ann. de Chim. July*, 1812,) a class of salts before that time almost wholly unknown. These experiments prove, 1. That the nitrous acid forms with the oxide of lead three different compounds, namely, a neutral nitrite and two subsalts, in one of which the acid is combined with twice as much base as in the neutral nitrite. This circumstance proves that the acid must contain a quantity of oxygen, which is a multiple of the oxygen in the base by an even number; and as that number can neither be 2 nor 6, it must necessarily be 4.

2. I showed how a neutral nitrite of ammonia may be produced, and I have proved that a solution of this salt, not too concentrated, exposed to a temperature of from 104° to 122°, is decomposed with effervescence, disengaging pure azotic gas without any change in the neutrality of the salt. This could not happen unless the acid, supposing azote to be its radicle, neutralized a quantity of base, the oxygen in which amounts to $\frac{1}{3}$ of that in the acid, or to $\frac{1}{4}$ if we suppose *nitric* to be the radicle. In that case the series of the oxidation of azote will be 1, 2, 3, 5, and that of nitric 1, 2, 3, 4, 6; and here, as is the case with sulphur and arsenic, the acid in *ic* contains the radicle combined with $1\frac{1}{2}$ times the quantity of oxygen that the acid in *ous* has. According to this determination, nitrous acid is formed when one volume of oxygen condenses $3\frac{1}{3}$ volumes of nitrous gas; that is to say, that one volume of azote combines with $1\frac{1}{2}$ volume of oxygen. Hence this acid is composed by weight as follows:—

Radicle	16·55	or	Azote	37·41
Oxygen	83·45		Oxygen	62·59
	100·00			100·00

I shall now add some proofs of the justness of this determination, by showing that what Gay-Lussac and Davy considered as nitrous acid could not be that acid in reality. One of the best methods of doing this is to examine what passes when lead is dissolved in neutral nitrate of lead, so as to convert the nitric acid of the salt into nitrous acid. (See *Ann. de Chim. July*, 1812.) To avoid long circumlocutions, I shall here employ a simple and short method of expressing determinate combinations, which I always use in my annotations.* Let Az = azote,

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* I shall take the liberty of giving a short explanation here of this method, which is founded on something very analogous to the corpuscular hypothesis of Dalton. It is known that bodies in their gaseous state either unite in equal volumes, or one volume of one combines with 2, 3, &c. volumes of the other. Let us express by the initial letters of the name of each substance a determinate quantity of that substance; and let us determine that quantity from its relation in weight to oxygen, both taken in the gaseous state, and in equal volumes; that is to say, the specific gravity of the substances in their gaseous state, that of oxygen being considered as unity. I have made choice of oxygen for unity, because it constitutes, as it were, the central point of chemistry, and because it enters more frequently than any other substance into compounds in various proportions, and ought therefore to be the easiest to add or subtract. It is obvious that this comes to the same thing as Mr. Dalton's weights of atoms; but I have the advantage over him, of not founding my numbers on an hypothesis, but upon a fact well known and proved.

When two bodies have the same initial letter, I add the second letter; and should that also be the same, I add to the initial the first consonant of the word that differs. In the class of combustibles which I call metalloids, I use only the initial letters. For example, S = sulphur, Sn = tin (*stannum*), St = antimony (*stibium*), C = carbon, Cu = copper (*cuprum*), M = muriaticum, Ms = magnesium, Mn = manganese, &c. This is all that is necessary

N = nitric, O = oxygen, and P = lead (*plumbum*). $Az + 5\ O$, according to Sir Humphry Davy, is nitric acid; and $Az + 4\ O$, nitrous acid. According to the result of my experiments, nitric acid ought to be represented by $N + 6\ O$, and let us write $Az\ \overset{5}{O}$, $N\ \overset{6}{O}$, &c. instead of $Az + 5\ O$, nitrous acid by $N + 4\ O$. To abbreviate these signs a little, $N + 6\ O$.

The formula which represents the nitrate of lead is then $Az\ \overset{5}{O} + O\ P$, and that which represents the neutral nitrite must be $Az\ \overset{4}{O} + P\ O$. Hence it follows, that in order to reduce the nitric to nitrous acid we must add another P, and we shall have $Az\ \overset{4}{O} + 2\ P\ O$. There will be no disengagement of nitrous gas; and when the nitrate has dissolved as much lead as it originally contained, no nitric acid can remain in it. Every portion of lead added beyond that quantity will occasion a disengagement of nitrous gas, and will form a nitrite containing a still greater excess of base, namely, $Az\ \overset{4}{O} + 4\ P\ O$. Let us examine whether this happens or not.

In my experiments on the formation of nitrite of lead, I have proved that the solution of the lead was always accompanied by the *disengagement of nitrous gas.* This circumstance appears at first to give a decisive proof against the determination of Davy; but this proof is only apparent; for whatever be the composition of nitrous acid, it is easy to prove that its formation ought always to take place without the evolution of nitrous gas, in consequence of the property which the two acids have of forming subsalts in different degrees of saturation; and if we consider the evolution of nitrous gas as a necessary consequence of the formation of nitrous acid, it will not be possible to form any idea of the interior of the operation. The disengagement of nitrous gas depends upon another cause, which I have already pointed out in my memoir on the nitrites. (*Ann. de Chim. July*, 1812.) It is owing to the property which the neutral nitrites have of being decomposed at a temperature between 177° and 212°, with the disengagement of nitrous gas, and the formation of subnitrates; and this happens equally, though there be no lead to dissolve. This disengagement is much slower, if the solution be dilute; but in concentrated solutions it takes place with effervescence. We must, then, ascribe the evolution of nitrous gas in the above experiment to the decomposition of a small portion of nitrite immediately after its formation. To prove the truth of this opinion, I digested a solution of ten parts of neutral

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to understand these formulas. The only thing that remains to render the use of them more general, is to determine correctly the specific gravity of the I shall publish hereafter a dissertation upon this subject.

nitrate of lead with metallic lead in an apparatus proper to collect the gas which might be evolved. The apparatus was surrounded with hot water, into which I had put a thermometer. When the water acquired the temperature of 131° the lead began to tarnish, and at 140° it was covered with a white crust, and the liquid began to acquire a yellow colour at 167°. There was a sensible action between the lead and the liquor, but still without any disengagement of gas. At 176° bubbles of air slowly formed, and the action was increased. I then diminished the temperature to 167°, and I kept it nearly at that temperature for 36 hours. There had disengaged about $\frac{1}{50}$th of a cubic inch of gas, which was a mixture of nitrous gas and of azotic gas driven from the solution by the elevation of the temperature. The lead was entirely covered by a white crystalline crust; and a quantity of the same substance crystallized on the inside of the glass during the cooling. The liquor had a fine yellow colour, which it retained even when cold. On examining it, I found that it contained a small quantity of nitrate of lead not decomposed, and neutral nitrite of lead, to which it owed its colour. The white crystalline precipitate was the first subnitrate. When heated it lost 3 per cent. of water; and when raised to a red heat, 19 per cent. of nitric acid were disengaged, leaving 78 parts of yellow oxide of lead. This experiment proves that the nitrite may be formed without the disengagement of nitrous gas; and it is evident, that when nitrous acid is formed at the same time that the base increases in quantity, it is the nitric acid that ought in the first place to combine with that excess, because its affinity for the oxide of lead is stronger than that of nitrous acid. After this long but necessary digression, we will resume our examination of the decomposition of nitrate of lead by lead in the metallic state.

We have said that when $\mathrm{Az}\ \overset{5}{\mathrm{O}} + \mathrm{P\ O}$ has dissolved an additional P, it is converted into $\mathrm{Az}\ \overset{4}{\mathrm{O}} + 2\ \mathrm{P\ O}$; that is to say, when 100 parts of nitrate of lead have dissolved $62\frac{1}{2}$ parts of lead (a quantity equal to that previously contained in the nitrate), the whole is converted into nitrite: and when a greater quantity of lead, as, for example, 70 parts, is dissolved, a considerable quantity of subnitrite at a maximum must be formed, at the expense of the nitrous acid of the nitrite; but I have proved that 100 parts of nitrate of lead are capable of dissolving 77·9 parts of lead before any perceptible trace of subnitrite at a maximum can be discovered (*Ann. de Chim. July*, 1812); that is to say, that the same subnitrite, which is already formed when the liquor has dissolved $62\frac{1}{2}$ parts of lead, continues still to be formed till the liquor has dissolved 78 parts of lead. This experiment proves, then, that nitric acid has more oxygen to lose before it is converted into nitrous acid than is consistent with the determination of Davy. Hence it follows that the difference between nitrous and nitric acids must be greater than between Az + 4 O and Az + 5 O.

But if we suppose nitric acid to be N + 6 O, and nitrous acid to be N + 4 O, we shall find that it will afford a satisfactory explanation of all the phenomena.

To understand completely the experiment in question, we must divide the operation into three periods. The *first period* continues till one-half of the nitric acid is decomposed by a second quantity of lead dissolved. There is formed at first some first subnitrate and some neutral nitrite; then the subnitrate begins to be decomposed, and to be converted into subnitrite. At the end of this period the liquor contains first subnitrate $(N\overset{6}{O} + 2PO)$ and first subnitrite $(N\overset{4}{O} + 2PO)$. The *second period* terminates when two-thirds of the nitric acid are converted into nitrous acid, and one-third remains still undecomposed. The nitrate has then dissolved 1¼ as much lead as it contained at first; that is to say, 78 parts. The solution while cooling, deposites second subnitrate $(N\overset{6}{O} + 3PO)$ mixed with a great quantity of the first subnitrite $(N\overset{4}{O} + 2PO)$. It is to be understood that I suppose no nitrous gas to be disengaged. This disengagement produces no effect upon the ultimate results of this operation; but when it takes place the quantity of subnitrite at the end f the second period is diminished in consequence, while at the same time that of the subnitrate is increased. The *third period* terminates when the whole of the nitric acid is decomposed. The nitrous acid is then united with a maximum of oxide, and forms a subsalt, which may be represented by this symbol, $N\overset{4}{O} + 4PO$; that is to say, that the nitrous acid and base contain equal quantities of oxygen. The nitrate has now dissolved 2⅕ as much lead as it contained before the commencement of the experiment; that is to say, 137¼: but it is very difficult to carry the experiment so far, and that from two causes: the subnitrite at a maximum covers the lead with a crust, and that prevents the mutual action between the metal and the liquid; and if we add new lead, the subnitrite at a minimum which is in the solution being too dilute, and being mixed with a great quantity of subnitrite at a maximum, acts only very weakly upon the metal. In several of my experiments, I have without difficulty dissolved 127 or 130 parts of lead by 100 of nitrate of lead. M. Chevreul, who has repeated a part of my experiments, has succeeded in dissolving 135 parts of lead in 100 of nitrate in two successive experiments. (*Ann. de Chim. July*, 1812.*) This approaches pretty nearly to the true limit. In my experiments I always found that the liquor, which after an ebullition continued for 12 or 16 hours floated over the lead covered with subnitrite at a maximum, deposited among the crystals of this subnitrite a notable quantity of subnitrite at a minimum, and of course 130 parts of lead are less than the nitrate was actually capable of dissolving.

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* *M.* Chevreul made a set of experiments on the formation of nitrite of lead, *it* would appear, without any knowledge of my experiments; and he

It is clear that if nitrous acid were $Az + 4 O$, and azote were considered as a simple substance, the greatest quantity of lead which could possibly be dissolved by 100 parts of nitrate of lead would be 87 parts; since that quantity being dissolved, the quantity of oxygen remaining in the nitrous acid would be equal to that in the oxide of lead combined with the acid. If, on the

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has verified some of my numerical results. In a subsequent paper he has endeavoured to prove that notwithstanding the harmony of our results, I have deceived myself in the explanation which I have given of the phenomena. The essay of M. Chevreul has given me much pleasure, and his objections are really founded on experiments; but he has not perceived that if my explanation here was faulty, and if the one which he has substituted in its place were more correct, he would have refuted me not only in this point, but in every thing relating to chemical proportions; for these proportions never can be true, if a single exception to them exists. M. Chevreul endeavoured, for example, to determine the water of crystallization of the subnitrite at a minimum, which he considers as the neutral nitrite, and he finds it inferior in quantity to that determined by my calculations. We have seen that the subnitrite at a minimum is always accompanied by a subnitrate, as M. Chevreul has likewise observed. If the subnitrite collected at the end of the first period of the operation be examined we shall find two subsalts, the first a subnitrate, the second a subnitrite, mixed together. This mixture, when analysed, gives for residue the same quantity of oxide which the first subnitrate would have given if it had been pure; that is to say, that the first subnitrite, supposing it pure, would have given the same quantity of oxide that the mixture gave. But the subnitrite contains water of crystallization, and the subnitrate contains none. Hence when the mixture is analysed it will give more acid and less water than calculation indicates in the pure subnitrite. Hence M. Chevreul has made a just observation; but which, when all things are considered, does not lead to the consequence that he has drawn from it. This distinguished chemist observes, that the precipitate formed in the solution of subnitrite of lead by the addition of water is rather a carbonate than a subnitrate, produced by oxidation. I am obliged to contradict him in this point; for in none of my experiments did this precipitate ever dissolve with effervescence in nitric acid. M. Chevreul observes, likewise, that the subnitrite at a maximum analysed by him was not so much coloured as I had described it; and in fact I have been much surprised in my latter experiments at obtaining two kinds of crystals of subnitrate at a maximum, the one reddish, the other pale yellow with a slight tint of red. The existence of more than two subnitrites being contrary to the theory of chemical proportions, these two salts attracted my close attention. After a rigorous examination, I found that both were the same subsalt, the one variety crystallized more regularly than the other, as was obvious when they were viewed through the microscope. The pale subsalt is deposited during the process, and when a solution containing it is suddenly cooled. If we then heat the liquid, and allow it to cool slowly, the same subsalt crystallizes regularly in small prisms, forming red coloured groups. In repeating the analysis of this salt, I found that it contained water of crystallization amounting to 1·88 per cent.; and that 100 parts of nitrous acid are combined in it with 1237 parts of oxide of lead, or four times as much as in the neutral nitrite.

other side, azote is considered as an oxide, and the nitrous acid be represented by N + 5 O, the greatest quantity of lead that can be dissolved by 100 of nitrate of lead will be 111·45; while experiment shows that it can dissolve 130. I think, then, that in the present state of our knowledge what I have stated may be considered as a decisive proof that nitrous acid is composed of 1 volume (atom or proportion) of *nitric* and 4 volumes of oxygen; and that it may be represented by N + 4 O.

Let us now examine what the combinations may be, called *nitrous acid* by Gay-Lussac and Davy. It is well known that the more powerful acids can hardly ever be produced without the presence of some oxide to serve them as a base. This is the case with sulphuric, nitric, muriatic acids, &c. I have endeavoured to prove, likewise, that the oxides of the same radicle often combine together in the same manner as oxides with different radicles. If in the experiment of condensing oxygen gas by means of nitrous gas, nitrous acid may serve as a base for nitric acid, it is obvious that the two acids must combine in such proportions that each shall contain equal quantities of oxygen, or that the one shall contain 2, 3, &c. times as much oxygen as the other. If we suppose that the acids combine in such a proportion that both contain equal quantities of oxygen, this combination will consist of 2 volumes of nitrous gas, and 0·9, or almost a volume, of oxygen gas. This, in all probability, is the nitrous acid of Sir Humphry Davy. If to the preceding mixture we add another volume of nitrous gas, we shall have the nitrous acid of Gay-Lussac, in which the nitric acid contains twice as much oxygen as the nitrous acid.

Let us now turn our attention to ammonia. We know that its alkaline nature indicates the existence of oxygen in it; for, on the contrary supposition, one of the strongest saline bases would be an exception to the general rule, and would fail in its analogy to bodies of a similar nature. I have shown that ammonia, to judge from the quantity of it necessary to neutralise an acid, must contain about 46·88 per cent. of oxygen: but we have seen that the combinations which nitric acid forms with saline bases would not be proportional to the laws of chemical combinations, unless azote contained oxygen; and as azote is one of the constituents of ammonia, we see in consequence that the presence of oxygen in that alkali is rendered very probable, or rather is proved.

When I first convinced myself that ammonia must contain oxygen, I considered it as composed of azote, hydrogen, and oxygen, according to the experiments of Sir H. Davy. Some time after, the experiments of Berthollet, jun. of Henry, and Davy, proved that ammonia, when decomposed, furnishes a quantity of azote and hydrogen equal in weight to the portion of alkali decomposed. Of course this first conjecture was ill-founded. I then had recourse to another explanation, namely, that ammonia is the oxide of a metal, which metal, with a smaller quantity of oxygen than exists in ammonia, constitutes hydrogen; and with a greater quantity, azote. I thought that the electro-chemical difference between these two oxides might be explained by a change in the original electric modification of the metal. The quantity of oxygen in azote ought to be in a determinate ratio to that in ammonia: and I found that if we suppose that the radicle of the alkali combines with 1½ times as much oxygen in azote as in ammonia, then all the calculations on the proportions of constituents, both in the alkali and in azote, correspond in an admirable manner.

As for the oxygen of hydrogen, I found that it was very difficult to prove either its existence or its quantity; because the oxygen of the azote in ammonia being subtracted from that of the alkali, there remained only a quantity so exceedingly small that it does not exceed the difference exhibited by the most careful experiments—a difference always occasioned by the imperfection of our methods. It was therefore doubtful whether hydrogen contained oxygen or not; and experiments did not speedily promise to throw any light on the subject; but I am able at present to prove, in a manner that appears to me decisive, that hydrogen contains no oxygen, and that none of our calculations on the laws of chemical proportions gives us a right to suspect that it contains any.

Proofs of the non-existence of Oxygen in Hydrogen.

1. Carbonic acid gas condenses twice its volume of ammoniacal gas, and forms subcarbonate of ammonia.

2. This subcarbonate decomposes without effervescence the nitrate of lead, and without altering the neutrality of the nitrate which is in excess. The carbonate obtained and analysed is composed in such a manner that the carbonic acid contains exactly twice as much oxygen as the oxide of lead. It follows that if ammonia contains oxygen, two volumes of ammoniacal gas contain just half as much oxygen as one volume of carbonic acid; that is to say, that ammonia contains exactly one-fourth of its volume of oxygen gas.

3. Ammoniacal gas decomposed by electricity gives two volumes of gas, 1½ of which are hydrogen, and ½ volume azotic gas.

4. Azotic gas being an oxide, it follows that the oxygen which it contains is proportional to the additional quantities which it absorbs in the subsequent degrees of oxidation; that is to say, in order to become nitrous oxide gas. Now this ratio will be a quantity equal, or double, or one-half. Let us suppose, at first,

that azote contains only $\frac{1}{2}$ as much oxygen as that which it absorbs in order to become nitrous oxide gas. If we subtract this quantity from the whole oxygen which ought to exist in ammonia, there remains to be found in the hydrogen a quantity of oxygen surpassing it in weight. On the other hand, azote cannot contain twice as much oxygen as that which it absorbs in order to become nitrous oxide gas, because that double quantity weighs more than the whole of the azote itself. Hence azote can neither contain more nor less oxygen than a quantity equal to that which it absorbs in order to become nitrous oxide gas; but one volume of azotic gas forms nitrous oxide gas by combining with half a volume of oxygen gas. Therefore azotic gas contains half its volume of oxygen.

5. We have seen that two volumes of ammoniacal gas contain half a volume of oxygen gas, and produce one volume of azotic gas, in which half a volume of oxygen gas is contained. Of consequence no oxygen remains for the hydrogen gas.

It follows, then, that the oxygen supposed from calculation to exist in ammonia is precisely equal to that demonstrated by the same means to exist in azote. Therefore hydrogen cannot contain any oxygen.—Q. E. D.

But what, then, is the constitution of ammonia, if hydrogen be an element, and azote an oxide? I think it not unlikely that some chemists will lay hold of this circumstance to revive the ingenious fictions of Beccher and Stahl in favour of phlogiston, by making hydrogen act the part ascribed to phlogiston; but I do not think that this improbable hypothesis will gain much by the explanation of the composition of ammonia; for if combustibility depend upon the presence of hydrogen in the combustible body, it follows either that azote contains hydrogen, or that it is an exception to the general rule. We have no more reason to consider the radicle of ammonia as composed of a base unknown rendered combustible by hydrogen, than to form the same supposition with regard to potassium and iron; an opinion which has been alternately combated and defended by one of the most celebrated chemists of our days.

If azote, as we have seen, is composed of oxygen and a combustible radicle, ammonia must be composed of two combustible bodies, combined with a single portion of oxygen common to both; but I have shown (*Ann. de Chim.* lxxx. 36) that this is the kind of composition which characterizes the productions of the organic nature, from which ammonia itself always derives its origin. We may, then, consider it as the oxide of a compound radicle, as is the case with tartaric acid, sugar, &c. In these bodies the combination of the oxygen with one of their combustible radicles, to the exclusion of the other, occasions decomposition. We can no more say, then, that ammonia is composed of azote and hydrogen, than we can that sugar is composed of carbonic oxide and hydrogen, or of carbon and water; because the very instant that these last substances are formed the organic compound ceases to exist as such.

In the present state of our knowledge we must consider ammonia as the oxide of a compound radicle; and the formula expressing the composition of the radicle will be $N + 3H$, and that for ammonia $N\overset{3}{H} + O$. It is easy to see that by this explanation the phenomena presented by ammonia will cease to be anomalous, and that the analogy of ammonia with other bodies is perfectly restored.

We have, then, two combustible bodies, which, by uniting with hydrogen, produce compound and oxidable radicles; namely, *nitric* and *carbon.* A radicle composed of nitric and hydrogen constitutes the base of ammonia; a radicle composed of carbon and hydrogen constitutes the principal base of vegetable bodies; a triple radicle composed of carbon, nitric, and hydrogen, constitutes the base of animal bodies. It is by varying on the one hand the proportions of the constituents of the radicles, and on the other that of the degrees of oxidation, that nature with so few elementary principles produces that prodigious diversity of organic products.

What, then, is the amalgam produced by the decomposition of ammonia? Certainly not the ammonia itself combined with hydrogen gas and mercury, as Thenard and Gay-Lussac have concluded from experiments far indeed from being conclusive:* nor is there any other way of explaining this phenomenon, at once singular and perfectly analogous to what happens with the other alkalies, than to consider the amalgam as composed of mercury, and the radicle either of ammonia or of azote, though the last is the least probable. If, on the other side, it is the radicle of ammonia which is combined with mercury, we have the curious phenomenon of a compound metallic body.

Before leaving this subject I trust I shall be permitted to draw the attention of the reader to some circumstances relative to ammonia and its compounds, which are well deserving of consideration:—

1. How comes it that *nitric,* a body so strongly electro-

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* It was taken for granted in these experiments that an ammoniacal amalgam, formed in liquid ammonia, but afterwards fixed by a kind of congelation, ought to contain no water. It is easy to see how little force this argument of Gay-Lussac and Thenard ought to have, when mercury itself in ordinary circumstances cannot be deprived of all the water with which it is penetrated without being boiled. Yet in this amalgam, formed in the midst of water, and ten times less dense than mercury, a dryness so absolute is supposed to exist that it does not contain water enough to oxidize the infinitely small quantity of metal combined with the mercury.

negative, produces with hydrogen, which is feebly electro-positive, a radicle strongly electro-positive? It will be said, because the hydrogen in ammonia is combined with less oxygen than in water, and because Thenard and Gay-Lussac have observed that, when in organic compounds, the hydrogen is to the oxygen as in water, the compounds are neither acid nor alkaline; but when the quantity of oxygen exceeds this ratio the substances are acids. The observation of the French chemists is interesting, but it proves nothing; for in acetic acid, which is one of the strongest vegetable acids, the oxygen is to the hydrogen precisely in the same ratio as in water; and the sulphuret and telluret of hydrogen preserve, in spite of the presence of hydrogen and the absence of oxygen, the electro-negative nature of sulphur and tellurium; so that hydrogen does not appear of itself to determine any thing respecting the electro-chemical nature of a compound. There remains, therefore, something at the bottom of this fact which we do not understand.

2. Water is composed of 11·75 hydrogen and 88·25 oxygen, and nitric acid likewise of 11·71 nitric and 88·29 oxygen. In azote the nitric is combined with 1¼ times as much oxygen as in ammonia is combined with the hydrogen and nitric together; that is to say, that according to the estimations which appear at present the most correct, the weight of the hydrogen in ammonia is precisely one-half of that of the nitric. Is this merely accidental? or is there some connection of cause which occasions it? Can we suppose that nature, in modifying the electro-chemical state of the radicle of ammonia, produces from it, under different and unknown circumstances, sometimes hydrogen, sometimes the suboxide of nitric? Is it possible that the nitric, that is, the electro-chemical modification which renders it capable of forming an acid, never exists without oxygen; and that on that account azote, though a compound, baffles all our attempts to reduce it: and consequently the combustible radicle can never exist alone under any other forms than those of ammonium and hydrogen? If these ideas should one day become probable, what views would they not furnish us with respecting the regeneration of the atmosphere, and the production of azote among herbivorous animals, which find so little of it in their food, and yet furnish daily so great a quantity of it in their excretions? But it is easy to make conjectures. Perhaps I have already indulged in them too far, Deceitful probabilities are almost always more injurious to science than the advancement of absurdities, or inaccurate experiments.

Article IX.

Memoir on the Determination of the Specific Heat of the different Gases. By MM. F. Delaroche, M.D. and J. E. Berard.

(*Continued from* p. 292.)

Section III.

Determination of the Specific Heat of the Gases by another Process.

We might have repeated a greater number of times the experiments which led us to the foregoing results; but we conceived that such a repetition would have been useless. The agreement which exists between those of the same kind, and which we obtained in two experiments following each other, shows that they were not influenced by accidental causes of error; and if they were subject to a constant cause of error, no advantage would result from multiplying their number. It was therefore more essential to endeavour to arrive at the same results by a different process, susceptible of almost equal precision, and to compare these results with the preceding. Count Rumford has put this in our power, by publishing the ingenious method by which he determined the quantity of heat disengaged during the combustion of certain substances, and which he had himself announced as fit for determining the specific heats of the gases.

We therefore undertook some experiments of this kind, employing the same apparatus which has been already described, and which had been constructed long before we were acquainted with the experiments of Count Rumford. It answered all the conditions demanded by this celebrated philosopher. It was, as has been already observed, a metallic vessel full of distilled water, traversed by a flat serpentine, sufficiently long to allow the gas while passing through it to acquire the temperature of the water in the calorimeter. As the gas entered by the lower end, it deposited there the greatest part of its heat; so that this heat spread itself sufficiently equably through the whole water that filled the vessel. We determined the temperature of this water by means of a thermometer, the cylindrical bulb of which was almost as high as the calorimeter itself, and which gave us, of course, the mean heat of the whole liquid. Our gazometer, and the part of the apparatus for heating the gas, were equally fitted for the experiment.

Leaving every thing disposed as in the preceding experiments, we made a new set, proceeding in the same manner, with this difference, that instead of waiting till the temperature of the calorimeter was become stationary, we determined the quantity of gas which passed through the serpentine while that temperature rose a certain given number of degrees, setting out from a given point, and likewise the time necessary to produce this effect. We employed the results of these experiments to calculate the specific heats of the gases experimented upon, setting out from this simple principle, that the specific heats, *cæteris paribus*, must be inversely as the quantities of gas necessary to produce the same elevation of temperature. In order to make accurate experiments according to this principle, it was necessary to take certain precautions, and to make some corrections on the results, which it will be proper to point out.

Philosophers have long been of opinion that the quantity of heat disengaged during the combustion of different substances might be determined in this manner; but it was necessary to contend with a cause of error, which proved very injurious to the accuracy of the results. In proportion as the calorimeter became hot, the air and the surrounding bodies deprived it of a portion of its heat. Hence the whole heat of the burning body was not indicated by the rise in the temperature of the calorimeter. It was necessary to include the heat lost during the process, and this was very difficult.

In order to get rid of this inconvenience, Count Rumford conceived the happy thought of beginning the experiment not at the temperature of the surrounding air, but a little below it, and to allow the experiment to continue till the calorimeter was heated as much above the surrounding air as it had been below it at the commencement. By this method he made the heating of the calorimeter almost independent of the surrounding air. During the first part of the process the air would communicate heat to the calorimeter: but during the second part the calorimeter in its turn would give out nearly as much heat to the air as it had received from it at first.

According to this principle, we sunk the temperature of our calorimeter about 10° Fahrenheit below that of the air of the room; and causing a current of hot gas to pass through it, we did not begin our experiment till the temperature of the calorimeter was within 4° of that of the air of the room. We then began to reckon the number of cubic inches of gas necessary to raise the temperature 8° higher than the initial temperature. By this method we made two experiments at once; one which gave us the quantity of gas necessary to raise the calorimeter 8°; another which gave that which was necessary to raise it the 4 intermediate degrees. It is difficult to say which of these determinations led us to the most exact conclusions: the difference between them was never considerable, as may be seen by the table of our results.

The principle upon which these experiments depended was such that it would have been requisite, in order to be applicable to the object in view, that the gases during their passage through the calorimeter should suffer the same diminution of temperature. As this could not actually take place exactly, we have always by the rule of proportion brought the results obtained to what they would have been if that had taken place. We took for a measure of the cooling of the gases the difference between the temperature of the gases when they entered the calorimeter, and their temperature when they issued out, which we considered as equal to that of the surrounding air. This last estimate could not differ from the truth; for since the gas in passing through the calorimeter assumed the temperature of the water in that vessel, which during one-half of the experiment was below the temperature of the air, and during the other half as much above it, a compensation took place, which enabled us to take the temperature of the air as the mean heat of the gas during the whole process.

The same compensation corrected another error, which might have been occasioned by the condensation of aqueous vapour. The gas saturated with vapour at the temperature of the air would deposite a portion of it in its passage through the serpentine during the first half of the experiment; but as it would take up nearly the same quantity during the second half of the experiment, the general result would not be affected.

We have observed already that we could not entirely prevent the gas tube from communicating some heat to the calorimeter. It was necessary to estimate the influence of this cause of error. We ascertained, by two very correct experiments, that the quantity of heat yielded in this manner to the calorimeter in ten minutes was capable of raising its temperature 0·342° Fahrenheit; and as it was sensibly proportional to the time, nothing was more easy than to allow for it.

We now present the result of our experiments in the following table, quite similar to the preceding one, and therefore requiring no explanation :—

Names of the gases.		Number of cubic inches of gas that passed thro' the calorimeter.	Temperature and pressure to which the gases were subjected.		Cubic inches of gas reduced to the temperature of 32°.	Temperature		Degrees of heat lost in passing thro' the calorimeter.	Effect produced by the gas on the calorimeter, abstracting the influence of the tube.	Ditto supposing the gas subjected to a pressure of 29·922 inches of mercury.	Number of cub inches of gas which at the temp. of 32° barom. of 29·922 inches would have raised the calorimeter 7·2° if the gas in passing had lost 85 per cent.	Means.
			Therm.	Barom Inches.		Of the gas when it entered the calorimeter.	Of ditto when it left the calorimeter.					
Air	1 Ex.	4967·7	51·8°	29·843	4470·6	207·14°	52·948°	154·192°	6·669°	6·669°	5189·8	Cubic Inches 5077·5
		2453·3	51·8	29·843	2356·3	207·14	52·948	154·192	3·334	3·334	5141·0	
	2 Ex.	4876·1	51·8	29·843	4682·7	207·14	52·948	154·192	6·669	6·669	5094·6	
		2331·3	51·8	29·843	2239·1	207·14	52·948	154·192	3·334	3·334	4884·7	
Hydrogen	1 Ex.	5468·1	54·5	29·843	5250·8	201·90	54·397	149·503	6·498	6·498	5685·3	5682·9
		2728·0	54·5	29·843	2620·0	201·90	54·397	149·503	3·330	3·330	5624·4	
	2 Ex.	5492·5	54·5	29·843	5274·7	201·90	54·397	149·503	6·498	6·498	5712·2	
		2721·8	54·5	29·843	2613·8	201·90	54·397	149·503	3·330	3·330	5661·0	
Carbonic acid	1 Ex.	3948·5	51·4	29·843	3792·3	206·87	50·581	156·289	7·146	7·146	3903·3	3870·4
		1971·2	51·4	29·843	1893·1	206·87	50·581	156·289	3·573	3·573	3898·5	
	2 Ex.	3887·5	51·4	29·843	3733·7	206·87	50·581	156·289	7·146	7·146	3842·3	
		1940·7	51·4	29 843	1863·8	206 87	50·581	156·289	3·573	3·573	3837·4	
Oxygen	1 Ex.	5059·2	54·7	29·540	4831·0	205·34	54·801	150·539	6·563	6·608	5180·1	5208·7
		2520·4	54·7	29·540	2406·9	205·34	54·801	150·539	3·281	3·303	5163·0	
	2 Ex.	5125·9	54·7	29·540	4895·1	205·34	54·801	150·539	6·563	6·608	5248·4	
		2560·1	54·7	29·540	2444·8	205·34	54·801	150·539	3·281	3·303	5243·5	
Oxide of azote	1 Ex.	3637·3	48·2	29·920	3518·9	206·60	47·957	158·643	6·840	6·840	3839·9	3859·4
	2 Ex.	3643·4	48·2	29·920	3524·4	206·60	47·957	158·643	6·840	6·840	3847·2	
		1843·0	48·2	29·920	1783·2	206·60	47·957	158·643	3·420	3·420	3891·1	
Olefiant gas	1 Ex.	3039·2	50·0	29·528	2929·4	207·23	49·044	158·186	6·955	7·056	3090·5	3022·1
		1452·5	50·0	29·528	1403·6	207·23	49·044	158·186	3·497	3·528	2953·7	
	2 Ex.	3051·4	50·0	29·528	2940·9	207·23	49·044	158·186	6·955	7·056	3102·7	
		1446·4	50·0	29·528	1393·9	207·23	49·044	158·186	3·497	3·528	2941·5	
Carbonic oxide	1 Ex.	4735·8	48·2	29·666	4581·4	207·59	47·255	160·344	6·669	6·717	5155·7	5164·2
		2331·3	48·2	29·666	2255·0	207·59	47·255	160·344	3·334	3·353	5075·1	
	2 Ex.	4760·2	48·2	29·666	4604·6	207·59	47·255	160·344	6·669	6·717	5180·1	

According to the principles from which we set out, the specific heats of the different gases are inversely as the numbers in the last column of the table. Hence, reckoning the specific heat of air unity, we obtain the following numbers for the same volumes of the other gases:—

	Specific heats of equal volumes.	Ditto by the first method.
Air	1·000	1·000
Hydrogen	0·893	0·9033
Carbonic acid	1·311	1·2583
Oxygen	0·974	0·9765
Azote	1·000	1·0000
Oxide of azote	1·315	1·3503
Olefiant gas	1·680	1·5530
Carbonic oxide	0·983	1·0340

The agreement between the results obtained by these two processes is as great as could be expected from experiments founded upon principles so different. As we consider our first method as most exact, we shall not take the mean between the two; but satisfy ourselves with exhibiting the result of the second set of experiments, as a proof of the accuracy of the first set.

SECTION IV.

Specific Heat of the Vapour of Water, that of Air being considered as Unity.

Aqueous vapour being an elastic fluid which produces a very striking effect in many phenomena, it was of importance to obtain correct notions of its specific heat. It was scarce possible to make experiments on this fluid in a state of purity; because such experiments require the whole of the apparatus to be preserved at a temperature higher than that of boiling water. It was necessary to have recourse to a mixture of vapour and air; and even in this way of experimenting several difficulties presented themselves. It was necessary to employ temperatures somewhat high, if we wished the vapour to constitute a considerable portion of the mixture: both our apparatus and our process were accommodated to this kind of experiment. It was sufficient to observe at what temperature our calorimeter became stationary when acted upon by a current of air saturated with humidity, and by the same current of air in a state of dryness. We were able only to employ air saturated with vapour at the temperature of 104°. To have employed a higher temperature would have been very difficult: but at the temperature of 104°, such is the tension of vapour, that it forms $\frac{1}{15}$th of the volume of the air with which it is mixed. It is obvious that, operating upon so small a proportion, to obtain an exact result was scarcely possible: but the nature of our apparatus, and the great pains which we bestowed on the experiments, induce us to think that the following results are not very remote from the truth.

The air, when it came out of the gazometers, was mixed with a great quantity of vapour, and then passed through a serpentine tube surrounded with water, kept all the time of the experiment at the temperature of 102·2°. In that serpentine it deposited its excess of vapour, so that we may consider it as perfectly saturated with vapour at the temperature of 102·2°. The current was then heated in the steam tube, and passed through the calorimeter, as in the other experiments.

To prevent the air from depositing water while passing through the calorimeter, it was necessary to keep it always above the temperature of 102°: for this purpose it was sufficient to heat the air in the room where the calorimeter was to the temperature of about 86°. The air thus saturated with vapour made the calorimeter stand stationary at 106·34°, the ambient air being 85·64°. Hence the effect produced was 106·34° − 85·64° = 20·7°; from which it is necessary to subtract 3·6°, the effect produced by the gas pipe. There remains 17·1° for the effect produced by air saturated with moisture at the temperature of 102·2°. Now this air entered the calorimeter at the temperature of 207·14°, and came out at the temperature of 106·34°. Hence it cooled 100·8° to produce this effect.

As soon as this experiment was finished, there was substituted for the vessel in which the air became saturated with vapour, a serpentine tube surrounded with a mixture of pounded ice and salt, which continued during the whole of the experiment below 5°. Things being thus circumstanced, the experiment was repeated. The current of air thus dried lost in passing through the calorimeter 102·42° of heat, and kept the calorimeter stationary at 104·72°, the ambient air being 85·64°. The effect produced by this dry air, subtracting the heat communicated by the gas tube, was 15·48°. By the rule of proportion it ought to have been 15·12°, if the current of air had lost 100·8° in passing through the calorimeter, as was the case with the current of moist air in the preceding experiment.

The difference between these two experiments, made exactly in the same way, is 17·1° − 15·12° = 1·98°. Therefore 1·98° is the effect produced by the vapour.

During the continuance of this experiment the barometer stood at 29·906 inches; and the tension of the vapour, at 102·2°, is, according to Dalton's table, 1·988 inch. Of consequence, in the air saturated with vapour, the volume of air was to that of vapour in the ratio of 15·0 to 1. The effect of elastic *fluids* on the calorimeter being, in the same circumstances, proportional to the quantity which passes through, it follows, from the preceding experiments, that if a volume of dry air raise the temperature of the calorimeter 15·12°, an equal volume of vapour would raise it 29·7°, other things being equal. Hence, if the specific heat of air be 1·000, that of the same volume of vapour is 1·96, and of the same weight, 3·136.*

Section V.

Determination of the Specific Heat of Air subjected to different Degrees of Pressure.

After having established the relation which exists between the specific heat of the different gases, we endeavoured to ascertain that which exists between the specific heat of the same gas subjected to different degrees of pressure, or, which comes to the same thing, having different densities.

Does the specific heat of a gas increase with its density? and if it does, is the increase proportional to the density? The experiments of Gay-Lussac, the only ones, as far as we know, which are capable of throwing light on the subject, seem to decide the first of these questions in the affirmative; but they are neither sufficiently direct nor precise to give an answer to the second.

The first process employed to determine the specific heats of the different gases, appeared to us equally proper to furnish an accurate answer to these two questions. It was sufficient to circulate a current of the same gas through the calorimeter, sometimes artificially compressed, and sometimes subjected only to the pressure of the atmosphere, and to determine in both cases the point at which the temperature of the calorimeter became stationary. The following modifications in our apparatus enabled us to make these experiments with facility:—

We soldered to the stop-cocks, P and P′, (fig. 8, Plate X.) a tube in the form of a Y reversed. The two branches of the Y united in a common tube, I I, which, rising vertically, and passing through an opening in the roof of the room, opened above a tub, X, placed in the room above. We carried to this room the cylindrical glass vessels, A, A′, and formed the communication between them and the reservoirs, B, B′, by the long tubes which passed down through the floor. In this apparatus the height, H H, of the upper extremity of the tube, I I, above the mouth of the stop-cocks, P, P′, determined the pressure to which the gas was subjected. It amounted to 11·29 feet.† It is easy to see that the gazometers thus modified would act exactly as before.

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* This is calculated on the supposition that the weight of vapour is to that of air as 10 : 16. See Gay-Lussac, Ann. de Chim. lxxx. 218.

† To enable the reservoirs to bear this great pressure without danger, they were firmly bound round with cords.

It was sufficient to make these experiments on a single gas to be enabled to draw conclusions applicable to all the rest. We made choice of atmospheric air, as the most convenient, and the least likely to be altered. This enabled us to remove the bladders, and the globular vessels that contained them, and to establish an immediate communication between the gazometers and the calorimeter; and hence to dispense with lutes, which we should have found it difficult to make sufficiently tight to resist so great a pressure.

We will satisfy ourselves with giving the result of a single experiment, made with great care, because it was conducted in every respect in the same manner as all the preceding ones.

The gazometers were so disposed as to give in ten minutes an uniform current of 1898 cubic inches of gas (at the temperature of 41°, the barometer standing at 29·634 inches, besides a pressure of 11·29 feet of water, making in all a pressure equal to 41·654 inches of mercury). The experiment was conducted in a manner perfectly analogous to all those that were exhibited in the first table. The result was, that this current of compressed air maintained the temperature of the calorimeter at 28·811° above that of the surrounding air, making an allowance for the heat communicated by the gas tube: but this air entered the calorimeter at the temperature of 206·42°, and came out of it at that of 73·098°; so that it was deprived of 133·322° of heat during its passage through the calorimeter. If we reduce by calculation the results of this experiment to what they would have been, supposing it the same with the first experiment made on air in the first table, we find that under a pressure of 41·654 inches of mercury, a current of 2196·4 cubic inches of air, by losing 130·347° of heat, raise the temperature of the calorimeter 33·666°; while we see by the table that under a pressure of 29·154 inches, the same current, in the same circumstances, only raise the temperature of the calorimeter 27·761°. The ratio between these two numbers being that of the specific heats of the gases, it follows that if the specific heat of air, under the pressure of 29·154 inches, be 1·000; that of the same volume of air, under the pressure of 41·654 inches, will be 1·2127.

We repeated these two experiments with the greatest possible care, one with air in its usual state, the other with air subjected to the artificial pressure. They have given us, after having made all the calculations, the ratio between the specific heat of air under the pressure of 29·154 inches, to that of air under the pressure of 41·654 inches, 1 : 1·2665. Taking the mean of these two determinations, we obtain the ratio of 1 : 1·2396.

Hence it is evident that the specific heats of equal volumes of gases subjected to different pressures are not equal, but differ materially from each other. It is evident also that the ratio between the specific heats is not that of the densities; for the ratio of the pressures to which the air in these experiments was subjected is 1 : 1·3583, obviously different from that of the specific heats 1 : 1·2396.

If we attend to the weight, it results, from the same experiments, that the specific heat of a mass of air increases with its bulk, without being proportional to it.

The experiments on the different gases having been made under different pressures, it was necessary, to make them capable of being compared together, to reduce the results to what they would have been supposing them all made under the same pressure. The experiments described in this section furnished us with the means of making this correction; for since we know that an augmentation of pressure, in the ratio of 1 to 1·3583, increases the specific heat of the same volume of air in the ratio of 1 to 1·2396, we may deduce from these ratios that of the specific heats of two equal volumes of air subjected to pressures of a different ratio from those that have been stated. We may, without running the risk of committing an error of any great importance, employ for that purpose the following proportion: $(1{\cdot}3583 - 1 = 0{\cdot}3583) : (1{\cdot}2396 - 1 = 0{\cdot}2396) :: \left(\frac{D}{P} - 1\right) : \left(\frac{x}{c} - 1\right)$; in which D and P denote the two different pressures; c, the known specific heat, corresponding to the pressure P; and x, the specific heat sought, corresponding to the pressure D. This formula is founded on the supposition that the differences between the specific heats are proportional to the differences of the corresponding pressures, which may be considered as true when the question relates to small differences. We employed the same formula for all the gases, because the law, according to which their specific heats vary according to different pressures, if it be not the same for all, must, at least, be very nearly the same.

(To be continued.)

Article X.

General Views of the Composition of Animal Fluids. By J. Berzelius, M.D. Professor of Chemistry in the College of Medicine at Stockholm.

(Continued from p. 208.*)*

Of Bile.

It is well known that the elder chemists considered the bile as an animal soap composed of soda and a resin. The accuracy of this opinion had often been questioned, owing to the very small proportion of soda; and lately our skilful contemporary, Thenard, has published an analysis of bile, in which he gives as its component parts, soda, a peculiar matter named by him *Picromel*, and a resin, which, united, produce a fluid that has the taste and other distinguishing properties of this secretion. Nevertheless, I am convinced that there is no such resin as Thenard and his predecessors have described. I shall not here relate my experiments on this supposed resin in particular, but shall give the result of my inquiries on the bile itself, which will enable the reader to confirm or reject my opinions, according as he finds them founded on accurate experiment.

The substance which is peculiar to bile has an excessively bitter taste followed by some sweetness; the smell is also peculiar, and the colour in most animals varies from green to greenish yellow. It is soluble in water, and its solubility is not in the least promoted by the alkali of bile, since, when this is neutralized by any acid, the peculiar matter does not separate: it also dissolves in alcohol in all proportions. Like the albuminous materials of the blood of which this peculiar matter is composed, it will unite with acids, producing compounds of two degrees of saturation, and hence, of solubility. The acetous acid, which gives soluble compounds with the albumen of the blood, does the same with the peculiar matter of the bile; and hence this matter is not precipitated on adding this acid to bile, though it falls down on the addition of the sulphuric, nitric, or muriatic acids. It is this sparingly soluble compound of biliary matter with a mineral acid which has been mistaken by many chemists for a resin; since it possesses the external characters of a resin, melts when heated, dissolves in spirit of wine, and is again precipitated (in part at least) by the addition of water. The alkalies, alkaline earths, and alkaline acetates, decompose and dissolve it: the former, by depriving it of its combined acid; the latter, by furnishing it with acetous acid, which renders it soluble in water.

The peculiar matter of bile will also combine with many metallic oxides into a pulverulent mass; and the above described resiniform compound of this matter, and any of the mineral acids, often form with the same oxides a substance like a plaster, resembling in this respect also the true resins.

The degree of insolubility possessed by these compounds of acid and biliary albumen varies both according to the species of animals, and also according to the length of time that the bile has been extracted; for the longer it has been kept, the more solubility these compounds acquire; but in this case I have always found that by pouring in a fresh quantity of acid, and slowly evaporating the mixture, the resinous matter falls down as the supernatant liquor becomes more acid.

The biliary matter may be obtained pure in the following way: mix fresh bile with sulphuric acid diluted with three or four times its weight of water; a yellow precipitate of a peculiar nature first appears, which must be allowed to subside and be removed; then continue to add fresh acid as long as any precipitate is formed; heat the mixture gently for some hours, and afterwards decant the fluid part, and thoroughly edulcorate the green resin which is left. This resin reddens litmus, and is partially and sparingly soluble in water. It may be deprived of its acid in two ways: one of them is by digesting it with carbonate of barytes and water, whereby the carbonate is decomposed, and the water forms a green solution possessing all the peculiar characters of bile: the other way is by dissolving it in alcohol, and digesting the solution, either with carbonate of potash, or carbonate of lime, till it no longer reddens litmus, and then evaporating it to dryness. Either of these methods will give the pure biliary matter, and there are also other ways of obtaining it, which I have described in my work on Animal Chemistry, vol. ii. p. 47.

This peculiar biliary matter, when pure, resembles exactly entire desiccated bile. Being soluble in alcohol it might be supposed that it would dissolve in ether; but this is not the case; for ether only changes it to a very fetid adipocirous substance, exactly as it acts upon the albuminous matter of the blood. One circumstance relating to the biliary matter has much surprised me, which is, that it gives no ammonia by destructive distillation. Therefore it contains no azote; but what can have become of the azote of the albuminous matter of the blood? for no vestige of azote is found in any other of the constituent parts of the bile, nor does bile contain any ammonia.

The following is the result of my analysis of bile:—

Water	907·4
Biliary matter	80·0
Mucus of the gall bladder, dissolved in the bile	3·0
Alkalies and salts (common to all secreted fluids)	9·6
	1000·0

2. *The Saliva.*

The saliva is one of the fluid secretions which contain more water than the blood. When first discharged from the mouth it holds suspended a mucus which is not dissolved in the saliva, but gives it its frothy quality. This mucus gradually subsides from the saliva when kept in a cylindrical vessel, and with more ease when previously diluted, after which the supernatant saliva may be decanted off.

Saliva is composed of

Water	992·9
A peculiar animal matter	2·9
Mucus	1·4
Alkaline muriates	1·7
Lactate of soda and animal matter	0·9
Pure soda	0·9
	1000·0

Two of these ingredients require further notice.

The peculiar animal matter of the saliva is obtained by adding spirit of wine to dried saliva; which dissolves the muriates, lactates, &c. The soda which remains in the insoluble portion is then extracted by fresh spirits slightly acidulated with acetous acid. The residue is mucus mixed with the peculiar salivary matter, which last may be dissolved by water, leaving behind the insoluble mucus.

This peculiar matter is therefore soluble in water, but not in spirits of wine. The solution in water, when evaporated to dryness, leaves a transparent mass that easily again dissolves in cold water. This solution is not precipitated either by alkalies, or acids, or subacetate of lead, or muriate of mercury, or tannin; neither does it become turbid by boiling.

The *mucus* of the saliva is readily procured by mixing saliva with distilled water, from which the mucus gradually subsides, and it may then be collected on the filter and washed. In this state it is white, and would seem to contain phosphate of lime mixed with it. This mucus is quite insoluble in water; it becomes transparent and horny in the acetic, sulphuric, and muriatic acids, but does not dissolve in them, and the alkalies separate nothing from them. The mucus therefore contains no earthy phosphate, though its appearance would lead to suspect this earthy salt. It dissolves in caustic alkali, and is again separated from it by the acids. A small proportion escapes the action of the alkali, but yields to muriatic acid, and is not separable from this acid by an excess of alkali. The mucus of the saliva is very easily incinerated, and though no phosphate of lime is detected in it by the acids in its natural state, a considerable portion of phosphate appears in the ash after combustion.

Is this mucus secreted in the salivary glands, or is it only the common mucus of the mouth? The latter appears most probable, though I confess that the large quantity of this mucus contained in the saliva, and the great difference between its chemical properties and those of the nasal mucus, throw some doubt on this opinion.

It is this mucus that produces the tartar of the teeth, which at first is only mucus precipitated on the surface of the teeth and adhering to them, but soon it begins to decompose, its colour changes by the influence of the air from white to yellow or greenish, the warmth and moisture of the mouth contribute to complete the decomposition, and the same earthy phosphates which are produced by oxidation and combustion in open fire are here formed and slowly deposited on the surface of the tooth by a slower but a similar process. The tartar is, therefore, as it were, the *ash* of mucus crystallized on the tooth, and this, as is well known, will in length of time form very large incrustations. I have found it to consist of the following substances:—

Earthy phosphates	79·0
Mucus not yet decomposed	12·5
Peculiar salivary matter	1·0
Animal matter soluble in muriatic acid	7·5
	100·0

3. *The Mucus of the mucous Membranes.*

I shall premise some remarks on the term *mucus,* as applied to animal chemistry. It properly signifies the mucus of the nostrils; but many chemists have extended it to other substances found in the animal fluids; so that Jordan, Bostock, Haldat, and others, reckon it among the constituents of these fluids. None of these chemists has considered mucus, used as a general term, as identical with the nasal mucus, or, if they have thought so, it has been a very great error. I must now mention that there is no such principle as the mucus of animal fluids, the substance so considered being in reality lactate of soda mixed with the animal matter that always accompanies it: but if it did exist as a separate principle, some other term should have been used, to distinguish it from the mucus of the nostrils, which is very different.

The chemists who have the most attended to the analysis of mucus have been Messrs. Bostock, Fourcroy, and Vauquelin; but none has given a very satisfactory account of its properties. The two latter chemists, who have published a long memoir on animal mucus, have too much generalized the characters peculiar to nasal mucus, in attempting to extend them to the mucus of the intestines and gall bladder, for example, where they are totally inapplicable.

The mucus of mucous membranes is produced from the same secretory organ throughout the body, and possesses every where the same external characters which constitute *mucus:* but in chemical properties the mucus of different organs varies considerably, according to the required use in protecting these organs from the contact of foreign substances. Thus the mucus of the nostrils and trachea, which is intended to protect these membranes from the external air, differs from that of the urinary bladder, which is to preserve this organ from the contact of an acid liquor, or from that of the gall bladder, whose contents are alkaline.

The animal matter peculiar to mucus is the same in all cases, and has the following properties: it is insoluble in water, but is able to imbibe so much of this fluid as to become more or less transparent, semi-liquid, or what is termed *glairy*. If in this state it be laid on blotting paper, and the paper changed as it becomes wet, the mucus may be deprived of the greater part of the moisture which it had absorbed, and will then lose most of its peculiar properties. Mucus is not coagulable by boiling, it becomes transparent when dry, and generally resumes its mucous character on adding fresh water, but there is great difference in this property.

The liquid part of mucus, or that fluid which the proper mucous matter imbibes, and to which it owes its fluidity, is the same as the liquid that remains after the coagulation of the serum.

My experiments give the following constituent parts of nasal mucus:—

Water	933·7
Mucous matter	53·3
Muriate of potass and soda	5·6
Lactate of soda, with its accompanying animal matter	3·0
Soda	0·9
Albumen and animal matter insoluble in alcohol, but soluble in water; along with a trace of phosphate of soda	3·5
	1000·0

Nasal mucus, when just secreted, contains a greater proportion of water than above stated. It is very fluid; and gives by evaporation only 0·25 per cent. of solid matter. There is reason to suppose that its peculiar animal matter is first dissolved in the free alkali, but is gradually precipitated as the alkali becomes carbonated by the contact of the respired air. The mucus which I analysed was of such a consistence that the whole quantity fell out on inclining the vessel that contained it.

The proper mucous matter of the nose has the following properties: immersed in water it imbibes so much moisture as to become transparent, excepting a few particles that remain opake: it may then be separated by the filter from the rest of the water, and may be further dried on blotting paper till it has again lost nearly the whole of the moisture it had imbibed. Mucus thus, dried will again absorb water when immersed in it, and resume its transparency; and this alternate wetting and drying may be repeated an indefinite number of times, but it thus gradually becomes yellowish and more resembling pus. Five parts of recent mucus absorbed by 95 parts of water produce a glairy mass which will not pour from a vessel. When mucus is boiled with water it does not become horny, nor does it coagulate; the violent motion of ebullition rends it in pieces, but when the boiling is discontinued, it is found collected again at the bottom of the vessel, and nearly as mucous as before. I should observe, however, that this mucus naturally contains a little albumen, which must first be extracted by cold water to enable the remaining mucus to exhibit the above-mentioned appearances. The nasal mucous matter dissolves in diluted sulphuric acid: when the acid is concentrated, the mucus is carbonized. Nitric acid at first coagulates it, a number of yellow spots being dispersed through the coagulum; but by continuing the digestion it softens, and is finally dissolved into a clear yellow liquid containing none of that yellow substance which I have described under fibrin.

Acetous acid hardens mucous matter, but without dissolving it, even in a boiling heat. Caustic alkali at first renders mucous matter more viscous, and afterwards dissolves it into a limpid flowing liquid. Tannin coagulates mucus, both when softened by the absorption of water, and when dissolved either in an acid or an alkali.

The Mucus of the Trachea, as far as I have been able to examine, possesses the same properties with the preceding. The first morning expectoration often contains bluish or dark coloured flocculi, which will imbibe 20 times their bulk of water, and sometimes become thereby so perfectly transparent as hardly to be distinguished in the surrounding water. Acids and alkalies act upon them as on nasal mucus.

The Mucus of the Gall-bladder much resembles that of the nostrils, but is more transparent, and is always tinged yellow by the bile. When dried it will again soften in water, but loses part of its mucous property. Biliary mucus dissolves in alkali, and its fluidity increases in proportion to the quantity of the latter. If this solution is exactly saturated with an acid, the mixture becomes slightly turbid, and of a consistence to be drawn out in threads. All the acids produce with biliary mucus a yellowish coagulum that reddens litmus. The coagulum formed with the sulphuric acid may be restored to its mucous properties by exact saturation with an alkali. Alcohol coagulates this mucus into a very yellow granular mass, to which the mucous property cannot be restored. A similar mass is often found in the adipocirous biliary concretions; and it is remarkable that it may be produced from mucus by alcohol, as from biliary matter by ether.

Bile itself is often of such a mucous consistence as to be drawn out in threads. This is owing to the presence of mucus dissolved in the alkali of the bile. A very little of any acid (the acetous for example) precipitates the mucus, and destroys the viscosity of the bile. Alcohol has the same effect.

Former chemists seem to have regarded this property of bile as owing to the presence of albumen, the existence of which has been considered as demonstrated by the precipitate caused by adding acetic acid or alcohol. From what I have already mentioned, it is clear that no precipitate produced by acetic acid can be albumen, since the latter is soluble in this acid; and as bile is not disturbed by prussiate of potass or by tannin after the precipitate by acetous acid is removed, this is a proof that no albumen can be contained in bile. The following experiment is conclusive that the supposed albumen of bile is only mucus: mix some bile with very weak sulphuric acid, drain on a filter the yellow precipitate thus formed, and then digest it with a saturating quantity of carbonate of soda and water, and the precipitate will be changed to a mucus, which will be more or less glairy, according to the quantity of water employed.

The Mucus of the Intestines accompanies the excrements, in which it often forms long and transparent filaments. When once dried, the addition of water will not restore its mucous property, but alkalies produce this effect, though without rendering it transparent.

The Mucus of the Urinary Passages accompanies the urine, in which it is partly dissolved, and partly suspended mechanically. The latter portion is generally too transparent to be distinguished by the eye; but it may be exhibited by letting the urine remain awhile at rest, decanting the fluid portion, and drying the mucus on a filter. This loses its mucous property totally by desiccation, and then often becomes rose-coloured, owing to the presence of uric acid, and appears to be crystallized. It softens a little in water. The urinary mucus is easily soluble in alkalies, and is not separated from this solution by acids. Tannin separates it in white flocculi. I shall return to this subject under the analysis of urine.

4. *Fluids of the Serous Membranes.*

It is well known that the surface of serous membranes is always moistened by a liquid, which in a state of health is never secreted in quantities sufficient for analysis; it is therefore only during a dropsical state of these membranes that we can gain any knowledge of its properties. This fluid may be considered as serum deprived of from $\frac{2}{3}$ to $\frac{4}{5}$ of its albumen. It does not coagulate by mere boiling, but it gradually becomes turbid, and during the evaporation a coagulated mass collects. This appears to be albumen; but it has a sulphur-yellow colour. It is composed of

Water	988·30
Albumen	1·66
Muriate of potass and soda	7·09
Lactate of soda and its animal matter	2·32
Soda	0·28
Animal matter only soluble in water, with a trace of phosphates	0·35
	1000·00

The fluid whose analysis is here given was that of hydrocephalus,* which probably approaches nearer than any other of the morbid effused fluids to the natural state, on account of the short duration of the disease, and the little time to which the fluid is exposed to spontaneous change within the ventricles of the brain. The other dropsical fluids are in general more concentrated, which may arise either from the mere consequence of being long kept, or from the transudation of the serum of the blood that always occurs in the last stages of dropsy, and appears also to take place in the urine and cellular membrane.

5. *The Humors of the Eye.*

The quantity of these fluids that can be procured is so small that it is not easy to obtain a very exact analysis of them. However, my experiments have shown me that they bear a very close affinity with the other membranous fluids. Those of the eye are distinguished by being perfectly transparent and colourless; the other membranous fluids having a yellowish tinge. The humors

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* It gives me much pleasure again to fall into the track of Dr. Marcet's labours, who has analyzed many of these fluids with results so nearly approaching my own as to be a considerable confirmation of their accuracy, particularly as our experiments were made nearly at the same time, and without any knowledge of each other's operations. The following are Dr. Marcet's results:—

	Fluid of Spina bifida.	Fluid of Hydrocephalus internus.
Water	988·60	990·80
Muco-extractive matter, &c.	2·20	1·12
Muriates, &c.	7·65	6·64
Subcarbonate of soda	1·35	1·24
Phosphates, &c.	0·20	0·20

I may observe that the circumstance of Dr. Marcet's having found a greater quantity of soda is owing to the decomposition of the lactate, as well as to the presence of carbonic acid.

of the eye are not coagulated by boiling. Their composition is as follows:—

	Aqueous Humor.	Vitreous Humor.
Water	98·10	98·40
Albumen	a trace	0·16
Muriates and lactates	1·15	1·42
Soda, with animal matter soluble only in water	0·75	0·02

The crystalline lens has a peculiar and very remarkable composition. It has been considered as a muscle from the well-known experiment of M. Reil, who, on treating it with nitric acid, discovered in it a peculiar muscular structure; and Mr. Chenevix also found that its density and specific gravity increased towards the center. But its solubility in water is a sufficient proof that it is not a muscle, though to effect this solution it is necessary to break it down, and then it leaves undissolved a small portion of extremely pellucid membrane. This circumstance, added to that of the increasing density towards its center, shows that the structure of the lens is cellular, the cells being filled with pellucid matter of different degrees of concentration.

The composition of the lens I have found to be as follows:—

Water	58·0
Peculiar matter	35·9
Muriates, lactates, and animal matter, all soluble in alcohol	2·4
Animal matter soluble only in water, with some phosphates	1·3
Portions of the remaining insoluble cellular membrane	2·4
	100·0

The matter peculiar to the lens is remarkable. It coagulates by boiling, and the coagulum has all the chemical properties of the colouring matter of blood, except colour, which is entirely absent. When burnt, it leaves a little ash, containing a very small portion of iron. The liquor in which the coagulum is formed reddens litmus, has the smell of the humors of the muscles, and like them contains free lactic acid.

The perfect achromatic transparence of the lens, notwithstanding its similarity in chemical properties to the colouring matter of the blood, is well worthy of notice. The black pigment of the choroidea is a powder insoluble in water and acids, but slightly soluble in alkalies. When dried and ignited, it burns as easily as a vegetable substance, and the ash contains much iron. From these observations it may well be supposed that the circulating blood is decomposed on the interior surface of the choroidea, leaving there its colouring matter, which is required for the purposes of vision, and conveying the remainder to the inner part of the eye perfectly limpid and colourless. Need I add that the received opinion of the presence of gelatine and albumen in the lens is erroneous? The existence of free lactic acid in the humors of the lens proves nothing with regard to its supposed muscular structure; but only shows the presence of absorbing vessels to convey the products of the spontaneous decomposition of animal matter, one of the most important of which appears to be the lactic acid.

(*To be continued.*)

Article XI.

Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Magnetical Observations.

Latitude 51° 32′ 40″ North. Longitude West in Time $6''\frac{82}{100}$.

1813.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Sept. 18	8h 55′	24° 15′ 43″	2h 10′	24° 21′ 56″	6h 00′	24° 15′ 12″
Ditto 19	9 00	24 16 09	1 50	24 21 22	— —	— — —
Ditto 20	8 50	24 16 00	2 03	24 23 32	5 55	24 12 02
Ditto 21	8 55	24 14 56	2 15	24 20 33	6 00	24 15 52
Ditto 22	8 50	24 12 55	— —	— — —	6 00	24 14 10
Ditto 23	8 45	24 13 45	2 05	24 21 38	5 55	24 15 00
Ditto 24	8 52	24 13 42	2 00	24 22 40	5 57	24 16 02
Ditto 25	9 00	24 16 01	— —	— — —	6 00	24 15 44
Ditto 26	8 50	24 16 01	1 57	24 23 07	5 55	24 15 50
Ditto 27	8 50	24 14 16	2 00	24 22 48	— —	— — —
Ditto 28	8 45	24 13 56	1 55	24 22 56	5 55	24 17 00
Ditto 29	8 50	24 14 45	2 02	24 24 30	5 50	24 17 38
Ditto 30	9 03	24 20 00	2 15	24 23 16	5 50	24 17 08

Mean of Observations in Sept.	Morning	at	8h 53′	..... Variation	24°	15′	46″	West.
	Noon	at	2 02	..... Ditto	24	22	32	
	Evening	at	6 03	..... Ditto	24	16	04	
Ditto in Aug.	Morning	at	8 44	..... Ditto	24	15	58	West.
	Noon	at	2 02	..... Ditto	24	23	32	
	Evening	at	7 05	..... Ditto	24	16	08	
Ditto in July.	Morning	at	8 37	..... Ditto	24	14	32	West.
	Noon	at	1 50	..... Ditto	24	23	04	
	Evening	at	7 08	..... Ditto	24	13	56	

Mean of Observations in June.	Morning	at	8h	30′	..... Variation	24°	12′	35″	West.
	Noon	at	1	33	..... Ditto	24	22	17	
	Evening	at	7	04	..... Ditto	24	16	04	
Ditto in May.	Morning	at	8	22	..... Ditto	24	12	02	West.
	Noon	at	1	37	..... Ditto	24	20	54	
	Evening	at	6	14	..... Ditto	24	13	47	
Ditto in April	Morning	at	8	31	..... Ditto	24	09	18	West.
	Noon	at	0	59	..... Ditto	24	21	12	
	Evening	at	5	46	..... Ditto	24	15	25	

Magnetical Observations continued.

Month.		Morning Observ.					Noon Observ.					Evening Observ.	
		Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Oct.	1	8h	53′	24°	14′	56″	2h	03′	24°	25′	46″		Discontinued, from the shortness of the days.
Ditto	2	8	45	24	13	28	2	05	24	22	50		
Ditto	3	8	52	24	14	39	2	05	24	21	02		
Ditto	5	8	50	24	15	40	2	00	24	24	30		
Ditto	6	8	45	24	15	56	2	10	24	22	05		
Ditto	8	8	50	24	14	54	—	—	—	—	—		
Ditto	9	8	35	24	12	33	2	05	24	22	12		
Ditto	10	8	45	24	16	47	1	55	24	28	36		
Ditto	11	8	45	24	14	42	1	58	24	22	28		
Ditto	12	8	45	24	14	42	2	05	24	19	07		
Ditto	14	8	50	24	15	28	1	55	24	23	02		
Ditto	15	8	45	24	15	20	1	50	24	22	23		
Ditto	16	8	45	24	17	32	2	00	24	20	57		
Ditto	17	8	35	24	16	50	1	57	24	22	06		

Sept. 30.—The wind blew hard from the east, the weather fine; and at noon the needle became unsteady, and vibrated three minutes.

Oct. 10.—The variation was remarkable, the day fine, with a pleasant breeze from the west; fifteen hours after it blew very hard from the same point, with rain.

Oct. 15.—The wind blew strong from the south-west, with a drizzling rain; and the needle, at intervals, vibrated at noon five minutes.

Rain fallen { Between noon of the 1st Sept. / Between noon of the 1st Oct. } 0·961 inches.

Evaporation during the same period, 2·000 inches.

ERRATA IN NUMBER X. P. 302.

Sept. 3, for 24° 13′ 11″ read 24° 23′ 11″.

Sept. 5, for 24 12 15 read 24 22 15.

Article XII.

Some Mineralogical Remarks on Greenland. By Thomas Allan, Esq. F.R.S.E.

(To Dr. Thomson.)

SIR,

In your second Number you were so obliging as to insert some remarks on a collection of Greenland minerals which fell into my hands a few years ago. I then mentioned, that the Gentleman who collected them would be here this season; and I have now the pleasure to announce to you the safe arrival of Mr. Geisecké, after a residence of seven years and a half in Greenland, where he has been occupied in examining the coast and numerous islands, from Cape Farewell, in lat. 59¼°, all along the west side of the peninsula, to 76°.

It is with much satisfaction that I find the losses, which he sustained in consequence of the war, have, in a great measure, been repaired by his industry and perseverance, although they added one year more to his detention in the country. Mr. Geisecké is possessed of a great deal of interesting intelligence, not only with regard to the mineralogy and geology, but also the zoology and botany, of the country: the former were his principal objects; and from his accurate knowledge of every part of the science, we have reason to expect a great deal of most useful and interesting information, as he is about to publish an account of his travels in Greenland.

With his permission, I am now enabled to give you some information respecting cryolite, and the other new minerals mentioned in the note I formerly sent you. Cryolite is found only in one place, in a very remote and unfrequented quarter, in a fiord, or arm of the sea, distinguished by the name of *Arksut*, situated about 30 leagues from the colony of Juliana Hope, in South Greenland. It occurs imbedded in gneiss, in two thin irregular seams: one of these contains the pure white cryolite, and is entirely uncontaminated with any admixture; the other is wholly composed of the brown discoloured variety, mixed with galena, pyrites, &c. They are situated very near each other: the first is washed, at high water, by the tide, and a considerable portion of it is exposed, the superincumbent gneiss being removed. It varies from one foot to 2½ in thickness. From the degree of decomposition which it has undergone, this curious fossil could not be procured attached to the matrix, particularly as it was always separated by a thin layer of mica in a state of disintegration.

The mineral to which you have given the name of sodalite likewise occurs only at one spot, but in a more accessible situation, at Kanerdluarsuk, a narrow tongue of land upwards of three miles in length, in lat. 61°. It is found in an extensive bed, varying from 6 to 12 feet in thickness, and dipping south. It occurs between beds of mica slate; these rest on gneiss; and in the upper one the graphite, noticed in the list of minerals formerly sent you, is found disseminated. The coasts are here precipitous, but the land nowhere in this vicinity above 1000 feet high.

The allanite occurs in granite at Kakasoeitsiak, near Alluk, between Capes Discord and Farewell, on the east coast, the extreme point of Mr. Geisecké's travels in that direction; and he consequently was unable to revisit it. Of your analysis of these two minerals Mr. Geisecké had heard, by means of a Hull paper, and had them noted in his catalogue under the names you have given them. Besides these, Mr. Geisecké has been so fortunate as to find a great variety of other minerals, entirely new, specimens of most of which he had the kindness to place in my cabinet. It is not, however, my business to rob him of the satisfaction of making them known to the mineralogical world himself, which, besides, I could only do in a very imperfect manner. After arranging his affairs in Copenhagen, where he proposes to place a suite of the minerals of Greenland in the Royal Cabinet, he proceeds to his own country, with the intention of immediately preparing for the press.

Your readers will probably be anxious to know the kind of country Mr. Geisecké met with in Greenland. So far as he saw, the continent was entirely primitive, excepting the peninsula, of Norsoak, in 71°. The large island Disco is composed of trap rocks, resting on gneiss and mica slate. Hassen Island, and one or two others in that neighbourhood, are also composed of trap; and among the immense group of the Vrouwen Islands, which are principally primitive, he found a few small ones belonging to the floetz class.

The difficulties Mr. G. had to contend with were innumerable, and the dangers continual. He frequently had to walk 30 or 40 miles, carrying on his shoulders the fruits of his labour; and in going from one island to another, he had nothing to convey him but the miserable seal-skin boats of the country, and these always managed by women.

I have, therefore, more reason to be surprised at his being able to form a collection at all, than that his minerals should have been in the dirty and uncouth state in which I found them. He, however, informs me, that they were in a very different condition when they were sent from him; and has pointed out several specimens which he had carefully packed up at the time, although they were loose and dirty when they came into my hands. I am, Sir, your most obedient,

Edinburgh, Oct. 12, 1813. THOMAS ALLAN.

ARTICLE XIII.

Proceedings of Philosophical Societies.

IMPERIAL INSTITUTE OF FRANCE.

Account of the Labours of the French Institute for 1812.

(*Concluded from p.* 311.)

MEDICINE AND SURGERY.

AFTER twelve years of experiments, made in every civilized country, since the discovery of vaccination, the Class conceived that it would be useful to collect the result of the observations on an object so important to humanity. Another motive rendered this undertaking necessary: objections and doubts had been raised by well-informed men, whose testimony was calculated to influence public opinion. It has even been questioned, whether small-pox inoculation, considered as a preservative, and in some cases as a remedy for various diseases, was not preferable to vaccination, or at least entitled to be preserved as well as it.

MM. Berthollet, Percy, and Hallé, commissioners, undertook the necessary researches to satisfy the intention of the Society; and presented, by means of M. Hallé, a long report, which the Class ordered to be printed. They bring the different points of discussion to six principal questions. Under these different heads they unite, as far as possible, every thing that has been accurately ascertained respecting the effects of vaccination in Europe, and in the countries where Europeans have been able to introduce vaccination.

They have collected a great many facts, observed particularly in France, England, Italy, India, and America, and observed in individuals of all classes, constitution, mode of life, habits, and manners, exceedingly different from each other. On the other hand, they endeavour to estimate the value of the principal facts upon which have been founded the most plausible objections, which they neither attempt to elude nor conceal. Thus, by comparing together the observations, they have been led to the conclusions with which they terminate their report: namely—

That vaccination does not introduce into the body a matter capable of producing a remarkable disturbance, and which requires to be expelled by a movement similar to that which results from inoculation: that the eruptions which sometimes appeared at first were not owing to the cow-pox matter; but to other circumstances, in the midst of which these vaccinations were performed:

That the unfortunate results which sometimes occurred were owing to causes altogether foreign, which made their appearance during the course of vaccination, and owing entirely to the state of the patients:

. That the disorders following vaccination, when not owing to pre-existing diseases, have been very particular cases, depending upon circumstances peculiar to individuals; and that their number bearing no proportion to the immense number of observations exempt from accidents of any kind, no general consequence can be drawn from them:

That the unfortunate results, even supposing them incontestible, are more than compensated by the numerous instances of chronical and obstinate diseases which have been completely removed by vaccination; examples which, when compared with similar effects from inoculation, especially if we take into consideration the greater danger of inoculation, leave the superiority greatly on the side of vaccination:

Finally, that the preservative virtue of vaccination, when the virus has been properly taken, and the pock has proceeded properly, is fully as great as that of small-pox inoculation; while it possesses the immense advantage of circumscribing small-pox epidemics, and affords reasonable hopes of finally annihilating this dreadful scourge of humanity. *

M. Portal has published a new edition of his treatise on asphyxias; a work printed and circulated by order of government, for the instruction of the people, and which has probably saved the lives of thousands of citizens, since it has been circulated in France, and in all the rest of Europe, by the numerous translations that have been made of it.

M. Dumas, correspondent and Dean of the Faculty of Medicine at Montpellier, has published a considerable work, entitled *General Doctrines of Chronical Diseases*, in which he considers this important subject in the most general point of view. Not confining himself to the external forms of these diseases, he ascends to the principles of their phenomena, determining by analysis the simple affections of which they are composed, and which may be considered as their elements. An accurate comparison of acute and chronic diseases induces him to conclude that there is no constant character which separates these two classes of diseases. In his account of chronic diseases, he shows that the want of nutrition and emaciation are produced more speedily by diseases connected with respiration than with the organs of digestion. He shows the constant relation between certain external forms, and a disposition to peculiar chronic diseases. Hence he deduces the character of the physiognomy peculiar to each.

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* This important paper will be found in Nos. II. and IV. of our Journal.

The study of the revolutions natural to these diseases has made him perceive a period within which it is still possible to prevent their formation : different kinds of crises which succeed it, and what may render these crises advantageous or injurious : the different changes of acute into chronic diseases, and *vice versâ*, and the cause of these alternations.

The determination of the simple affections of which these diseases are composed, or of their pathological elements, appeared to him of the greatest importance; since it furnishes us, in some measure, with the means of simplifying them, by attacking these elements one after another, beginning with the most powerful. This fundamental point of view has enabled him to explain their formation, and to determine in a solid manner the method of treating them ; but for this purpose it was necessary to draw an accurate line between the essential elementary affections and the symptomatic.

Thus he has risen by degrees to the general phenomena, and has been able to deduce them from a small number of primitive affections. His theory of the formation of chronic diseases reduces itself to the relations of the elementary affections to each other, and to the system of organs which they occupy.

M. Dumas treats, in a manner which he considers as new, every thing that regards the general disposition to chronic diseases. He establishes a difference between the constitution and the temperament, which are sometimes opposed to each other ; and this opposition is the most direct cause of a tendency to chronic diseases. He estimates the influence of time of life by the relation between the elementary affections, from which results a disposition at every age to different kinds of diseases, modifications in the diseases common to each age, and changes advantageous or hurtful during the progress of each disease. He treats of the passions after analogous views. Each of them may be decomposed into a certain number of simple affections, which Metaphysics knows and enumerates.

Finally, M. Dumas arrives at his last part, which is that of the treatment. He shows the justice of his doctrine, by making it appear that all the approved methods of treatment are easily reducible to the principles which he has established. He finishes with some interesting observations on hereditary and on incurable diseases.

In an appendix, M. Dumas gives several examples of the manner in which he thinks the particular and detailed history of the elementary affections may be drawn up. A second work, which he promises, will establish and explain, by examples drawn from his practice, every thing difficult and abstract which this general doctrine contains.

AGRICULTURE AND TECHNOLOGY.

M. Tessier has presented us with the results of numerous experiments which he made to compare with each other the farina of different varieties of wheat, and the bread made from them. Wheats raised in the same field, ground in the same mill, baked by the same baker, and in the same oven, gave loaves very different in appearance, taste, and rapidity of drying; differences which may influence us in the preference given to each variety.

M. Parmentier, who is not prevented by his great age from devoting himself with the same ardour to every thing that affects the prosperity of agriculture, has given a sketch of the results obtained in the manufacture of syrups and conserves of grapes, from the time when his treatise on the subject drew to it the attention of agriculturists, and he shows all the advantages that may be derived from this method, now that he has brought it to perfection by experience.

The same author has published a new edition of his *Treatise on the Culture of Maize*, which received the prize in 1784 from the Academy of Bourdeaux, and which has been of essential service to the southern departments of France.

The art of rotation of crops consists in making it produce, by the succession of vegetables committed to it, and by the labour which the cultivation of them requires, every thing that it is capable of producing without ever injuring it. For some years it has been the principal object of research with those who employed themselves in promoting agriculture.

M. Yvart, correspondent, who has undertaken a great work on the subject, has this year submitted to the Class *An Historical Notice on the Origin of Rotation of Crops* (*assolemen*), *followed by an Explanation of the principal Motives and Means adopted to extend it in the French Empire.* This notice seems intended as an introduction to his great work. The researches of the author have shown him that the triennial rotation so common in Europe at present is a modern invention. This rotation, which consecrates the third year to the repose of the soil, has been introduced by laziness and poverty, and perpetuated by custom and ignorance. In the historical notice of Yvart, we see that all good farmers, both ancient and modern, of every country, have ascertained that the true repose of the soil consists in varying productions. Virgil has consecrated this precept:—

> "Sic quoque mutatis requiescunt fœtibus arva."—Georg. Lib. I.

But it was destined for the writers of our days to explain and support, by numerous examples, the advantage of proper rotations. M. Yvart, one of those who has paid most attention to the subject, has the merit of drawing all his from the soil of France itself. His instances are very numerous. He has given them for all climates, for all exposures, for all varieties of soil. He has shown that we may every where suppress the fallows, and introduce in their place a rotation of crops much more productive, and capable of retaining all the fertility of the soil.

M. Hassenfratz, divisionary inspector of mines, charged by government to publish a treatise on metallurgy, has submitted to the Class the first part of it, entirely devoted to the extraction of iron from its ores, on account of the importance of this metal for the arts, and the numerous works required by the different modifications of which it is susceptible. It has been found that the facts contained in this work are arranged in a natural order; that all the processes are explained with clearness; and that it may be considered as a general collection of every thing known respecting iron, either from books, from workmen, or ironmasters. This first part has been recently published.

Article XIV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Ulmin.*

Notwithstanding the various observations and experiments which I have had an opportunity of making on ulmin, it is only lately that I have been so fortunate as to meet with it in a state of purity, so as to be able to ascertain its real properties, and to assign it a true place among vegetable substances.

I received some time ago from Dr. Leach a specimen of a black matter which he had collected on an oak in the neighbourhood of Plymouth. It turned out, on examination, to be ulmin quite free from potash, a substance which had constituted a part of all the specimens formerly examined either by Mr. Smithson or myself.

It was in small lumps, mostly attached to each other by vegetable fibres. Its colour was a very dark brown, almost black It had little or no taste. It was considerably firmer than the ulmin mixed with potash, and of a greater specific gravity. I found its properties as follows :—

1. Nearly tasteless.
2. Dissolves very slowly, and in small quantity, both in water and alcohol. The solution is pale brown, and tasteless.

3. The aqueous solution is not precipitated by sulphate of iron, sulphate of copper, sulphate of zinc, or nitrate of silver.

4. Nitrate of mercury, and superacetate of lead, occasion brown flocky precipitates; but both of these salts were precipitated white by distilled water.

5. The acids occasion no precipitation in the aqueous solution.

6. The alcohol solution was precipitated dark brown by sulphate of iron and sulphate of copper.

7. When a weak solution of carbonate of potash is poured upon this ulmin, a dark-brown solution is immediately effected, which possesses all the properties formerly described in my paper on ulmin, in the first Number of the *Annals of Philosophy.*

I burned five grains of this ulmin in a platinum crucible. A white ash remained, weighing 0·27 grain. It contained no perceptible portion of alkali, but dissolved with effervescence in nitric acid, and was wholly carbonate of lime.

The most striking property of ulmin is its affinity for carbonate of potash. It is by means of carbonate of potash that it is usually so soluble in water. The acids throw it down, merely by depriving it of its potash. The metalline salts throw it down from its alkaline solution by a double decomposition.

II. *Ulmin from the Horse-Chesnut.*

Mr. Sowerby was so obliging as to send me a specimen of a black matter which he collected from a horse-chesnut (*Æsculus hippocampus*) growing in a garden in Southgate. In appearance and taste it resembled former specimens of ulmin very closely; but it was so much mixed with portions of the bark of the tree, that I could hardly obtain any quantity in a state of perfect purity. Ten grains of it (not free from bark) being burnt in a platinum crucible, left 1·44 grain of a white ash. This consisted chiefly of carbonate of potash mixed with a little carbonate of lime, and with a portion of silica, which I could not weigh, but estimated at about 0·04 grain. It was dissolved in nitric acid, and shot into small crystals of saltpetre. It appears, then, that this ulmin contains less potash than the ulmin from the elm.

Five grains of it being treated with water, formed a dark brown solution, and left behind a portion of black looking matter, which I found to be bark. This solution was not precipitated by acids, nor by sulphate of zinc, nor muriate of tin. Sulphate of iron threw down a grey precipitate; sulphate of copper, a green; nitrate of silver, a white; and acetate of lead, a yellow.

These differences from the ulmin of the elm seem to depend upon the smaller quantity of potash present, which prevents water from dissolving so great a quantity as to be precipitated by acids.

III. *Fall of Stones from the Atmosphere, near Chester.*

I received some weeks ago a letter from Chester, dated the

15th of September, containing the following information, which the writer says was first communicated to the public in a provincial newspaper. He does not give the date; but merely quotes the following passage from the newspaper, in the words of the anonymous writer of that article. "Last week, having occasion to go to Malpas (a village 15 miles from Chester), I witnessed a very singular phenomenon. About one o'clock in the day, from the great heat and the calmness of the air, I apprehended a thunder-storm, and supposed my apprehensions were going to be realised, when I beheld a bright cloud, out of which fell some large stones, which were soft and intensely hot at first, but afterwards acquired considerable hardness."

I am not aware that any of the stones in question have been brought to London. These phenomena have been of rare occurrence in Great Britain of late; but five or six examples of similar falls on the Continent, during the years 1811 and 1812, have been recorded, and the stones subjected to chemical analysis. As some of the results of analysis are curious, I have been intending to lay them before the readers of the *Annals of Philosophy*, but have hitherto been prevented by want of room.

IV. *Swedish Agriculture.*

I take this opportunity of correcting a very important mistake which occurs in my *Travels in Sweden*, relative to the quantity of grain produced annually in the kingdom. In page 426 of that work I have given an official table exhibiting the quantity of ground in tillage, and the annual produce in *spanns*; and I say, below the table, that the Swedish *spann* contains 28 kanns, or 4½ English wine pints. This statement I took from a Swedish dictionary, published at Stockholm in 1807.

But I have received a letter from a Swedish gentleman in London, stating the following to be the real amount of the Swedish measures, from his own personal knowledge, which I have no reason to call in question.

1. The Swedish tunn consists of two spanns heaped as much as can be laid on the top, and is the only lawful measure of the country. It amounts to

	French cubic inches.	English cubic inches.
	8310	9438·8
The Swedish kann is	132	149·9

Now a Winchester bushel is 2150·42 cubic inches: therefore the Swedish tunn, according to the preceding statement, is 4·3893 Winchester bushels, and the Swedish spann is 2·19465 Winchester bushels; so that these measures are above 40 fold greater than I made them. This removes the extraordinary barrenness, which appeared so very striking according to my original estimate; so that the tunnland (1¼ acre) produces annually rather more than eight Winchester bushels of corn. I thought it right to make this error known as soon as possible, to prevent any erroneous inferences from being drawn from the table in question.

V. *Sulphate of Soda.*

Want of room obliges me to defer for the present the continuation of the table of the constitution of the salts: but I think it necessary to point out a mistake in that part of the table which was inserted in the last Number of the *Annals of Philosophy.* Sulphate of soda (p. 294) is said to be composed of 1 integrant particle of sulphuric acid + 2 integrant particles of soda. From the note it will easily be seen that the reverse is the case. It is composed of 2 s. a. + 1 soda, and its weight, of course, is 10·7882. Hence the remarks made upon it in page 300, as an exception to Berzelius's rule, are erroneous. I have not yet met with any exception to his rule, except among the nitrates. These exceptions he admits, and endeavours to obviate by the ingenious theory published in this and the preceding Number of the *Annals of Philosophy.*

VI. *Electrical Oxides.*

The beautiful figures produced on paper by the oxidation of various metals with an electrical battery, cannot be effectually represented by engravings. Mr. Singer proposes to illustrate a few copies of his *Elements of Electricity* (now in the Press) with some real oxides, produced by his powerful apparatus. Those who desire such copies may secure them by an early transmission of their names to Mr. Singer.

VII. *Practical Chemistry.*

Dr. Thomson proposes next winter to give a Practical Course of Chemistry to a very limited number of young Gentlemen, who will reside in his house during its continuance. The Course will begin on the 1st of January, 1814.

ARTICLE XV.

Scientific Books in hand, or in the Press.

Mr. Kerrison is preparing for the Press An Inquiry into the Establishment and Progress of the Medical Profession in England, as it regards the Physician, Surgeon, Apothecary, General Practitioner, and Chemist and Druggist; with a Compendious Analysis of all the Charters granted to Physicians, Surgeons, and Apothecaries, tending to illustrate the Merits of the Bill about to be submitted to Parliament by the Associated Surgeon-Apothecaries of England and Wales.

Mr. W. Henley is about to publish a Series of Chemical Tables, intended to exhibit the Properties of all the present known Bodies, the Result of their Union, &c.; forming a Complete Abstract of the Science of Chemistry.

ARTICLE XVI.

METEOROLOGICAL JOURNAL.

1813.	Wind.	Barometer.			Thermometer.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
9th Mo.										
Sept. 17	S E	30·22	30·11	30·165	72	56	64·0	—		☾
18	S E	30·11	30·01	30·060	70	46	58·0	—		
19	E	30·01	29·92	29·965	72	43	57·5	—		
20	N E	29·90	29·89	29·895	66	45	55·5	—		
21	N E	29·91	29·87	29·890	70	50	60·0	·29	—	
22	N	29·96	29·90	29·930	68	48	58·0	—	6	
23	N E	30·01	29·96	29·985	64	54	59·0	—	·14	
24	N E	30·11	30·01	30·060	66	47	56·5	—	—	●
25	N E	30·11	30·03	30·070	62	51	56·5	·20		
26	N E	30·03	30·01	30·020	67	47	57·0	—		
27	E	30·01	29·94	29·975	66	44	55·0	—		
28	N E	—	29·94	—	60	47	53·5	—	—	
29	N E	30·12	—	—	62	47	54·5	·21		
30	N E	30·12	29·84	29·980	62	40	51·0	—		
10th Mo.										
Oct. 1	N E	29·84	29·68	29·760	61	41	51·0	—		
2	N E	29·70	29·67	29·685	59	49	54·0	—	—	☽
3	S E	29·80	29·70	29·750	62	48	55·0	·21	—	
4	S E	—	—	—	—	—	—	—	—	
5	W	29·78	29·65	29·715	66	54	60·0	—		
6	S W	29·80	29·77	29·785	66	52	59·0	—		
7	S W	29·52	29·47	29·495	65	51	58·0	—	—	
8	W	29·73	29·46	29·595	60	54	57·0	—	2·50	
9	Var.	29·43	29·42	29·425	61	49	55·0	—		
10	S W	29·44	29·15	29·295	65	45	55·0	—	4	○
11	W	29·46	28·93	29·195	60	48	54·0	—	—	
12	S W	29·75	29·55	29·650	60	41	50·5	—	—	
13	S W	29·55	29·34	29·445	58	52	55·0	—	·45	
14	N W	29·62	29·52	29·570	58	34	46·0	—	·29	
15	S	29·58	29·21	29·395	51	33	42·0	·28	·47	
		30.22	28·93	29·752	72	33	55·23	1·19	3·95	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

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ARTICLE XV.

REMARKS.

Ninth Month.—19, 20. Breezes by day: much dew. 21. Cloudy quite to sun-set: a few drops of rain. 22. A breeze, a. m. bringing clouds: p. m. a sudden shower: rain in the night. 23. Windy: showers. 24. Much wind. 25. Windy: cloudy. 26. Overcast a. m.: clear p. m.: a luminous twilight, with *Cirrus* and *Cirrocumulus*. 27. Morning twilight somewhat coloured: forenoon, overcast: clear, p. m.: and at sunset, fascicular *Cirri*, arranged from W. to E.: the wind being E. and nearly calm. After these appearances, lightning far to the S. E. and S. W. 28. Cloudy, with a few drops. 30. A pink twilight, with dense, coloured *Cirri*. For three days past, a steady N. E. breeze, with pretty much sunshine.

Tenth Month.—1. Overcast, a. m.: wind N. After sunset, *Cirrocumulus* passing to *Cirrostratus*, a corona round the moon, and a small meteor, which went W. 2. Overcast most of the day: a few drops, p. m. 3. *Cirrus*, with *Cumulostratus*: twilight opaque, orange coloured. The roads have become of late excessively dry, and the dust raised from them floats in great quantity in the air. 4. Early this morning began a steady rain, which continued till after sun-set. 5. Fine day: lunar halo. 6. Cloudy. 7. A considerable storm of thunder and lightning early this morning, followed by much rain. 8. Fair, a. m.: wet, p. m. 9. Fine day. 10. Wet, with a fair interval. 11. Wet, a. m.: fair, p. m. 12. The reverse of yesterday. 14. Fair. 15. Very wet.

RESULTS.

Prevailing Winds, Easterly, and drying, to the first quarter of the moon; soon after which they became Westerly, and brought much rain.

Barometer: greatest observed elevation . . 30·22 inches;
Least . . . . 28·93 inches;
Mean of the period . . . . 29·752 inches.
Thermometer: greatest height . . . . 72°
Least . . . . 33°
Mean of the period . . . . 55·23°
Evaporation, 1·19 inches. Rain, 3·95 inches.

The rain of the 4th inst. having put a conclusion to a fine season of some weeks' continuance, I availed myself of the opportunity of a journey made immediately after it, to ascertain, as far as I could, its extent. I found that it had rained from morning to night on that day all the way between London and York, also (by information from other travellers) as far north as the Tyne, and over the narrow part of the island, from Cheshire to Northumberland. It having been likewise a very wet day on the south coast, I conclude that probably the whole of England was, on this occasion, irrigated at once from an Atlantic current, which during the prevalence of the easterly breeze just before, had taken possession of the higher atmosphere, and which on that day arrived, in its progress of subsidence, near enough to the earth to part with its electricity, and displace the lower stream of air.

Tottenham, Tenth Month, 23, 1813. L. HOWARD.

ANNALS

OF

PHILOSOPHY.

DECEMBER, 1813.

ARTICLE I.

Biographical Account of Sir Isaac Newton.

(*Concluded from p.* 328.)

IN the year 1704, the *Treatise of Quadratures*, by Newton, was published. This treatise had been written long before, many things being cited out of it in Newton's letters of Oct. 24, and Nov. 8, 1676. It related to the method of fluctions; and that it might not be taken for a new piece, Newton repeated what Dr. Wallis had published nine years before without being contradicted, namely, that this method was invented by degrees in the years 1665 and 1666. But the editors of the *Acta Lipsica*, in their review of this book, (and the author of the review was conceived to have been Leibnitz himself) represented Leibnitz as the first inventor of the method, and said that Newton had substituted fluctions for differences, just as Fabri, in his *Geometriæ Synopsis*, had substituted movement for the indivisibles of Cavalleri. This accusation gave a beginning to the controversy. For Dr. Keill, in an epistle published in the Philosophical Transactions for September and October 1708, retorted the accusation, asserting that "all these things follow from the now so much celebrated method of fluctions, of which our Newton was doubtless the first inventor, as will be evident to any one who shall read his letters published by Dr. Wallis. Yet afterwards the same method was published by Mr. Leibnitz in the *Acta Eruditorum*; only changing the name and manner of notation."* Leibnitz, understanding this passage as a direct charge of plagiarism, complained of it as a calumny in a letter to Sir Hans Sloane, the Secretary, dated March 4, 1711, and moved that the Royal Society would cause Dr. Keill to make a public recantation. Dr. Keill chose rather to explain and defend what he had written; and though Newton was at first offended at his original paragraph, being apprehensive that it might occasion a controversy, yet when he was shown the accusation in the *Acta Lipsica*, he gave Keill liberty to maintain the opinions which he had advanced. Keill wrote a long letter to Sir Hans Sloane, in which he endeavours to demonstrate not only that Newton was the original discoverer, but that he had given Leibnitz so many hints of his method, that even a man of very ordinary abilities could hardly fail to make it out. This letter was sent to Leibnitz, who demanded that the Royal Society would put a stop to the accusations of a man too young to know what had passed between Newton and himself. Instead of making good his own accusation, as he ought to have done, that it might not be deemed a calumny, he insisted only on his own candour, and refused to tell how he came by his method. He said that the *Acta Lipsica* had given every man his due; that he had concealed the invention above nine years, that nobody might pretend to have been before him in it. He called Dr. Keill a novice who deserved to be silenced, and desired that Newton himself would give his opinion in the matter. Dr. Keill, in fact, had only repeated what Dr. Wallis had published 13 years before, and Newton had already given his opinion on the matter before the dispute began; and this opinion in all probability was the cause of the controversy, by giving origin to the severe and unjust treatment which the book had received in the *Acta Lipsica*.

The Royal Society being thus twice pressed by Mr. Leibnitz, and seeing no reason to condemn or censure Dr. Keill without inquiring into the matter; and that neither Newton nor Leibnitz (the only persons alive who knew and remembered any thing that had passed respecting these matters 40 years before) could be witnesses for or against Dr. Keill, appointed a committee, consisting of Dr. Arbuthnot, Mr. Hill, Dr. Halley, Mr. Jones, Mr. Machin, and Mr. Burnet, to search old letters and papers, and report their opinion on what they might find, and ordered the letters and papers, with the report of their committee, to be published. This publication was the famous *Commercium Epistolicum*, containing the letters of Oldenburg, Collins, Leibnitz,

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* Phil. Trans. 1708, vol. xxvi. p. 174. The passage occurs in a Latin paper, by Dr. Keill, on *The Laws of Centripetal Force.*

Gregory, and Newton. The committee reported, that it appeared to them that Newton had the method in or before the year 1669, and it did not appear to them that Mr. Leibnitz had it before the year 1677, a year after the communication of a letter from Newton to Leibnitz, in which the method of fluctions was sufficiently described for every intelligent person. They add, that the differential method of Leibnitz was the same as the fluctionary calculus of Newton, the notation excepted; that they regard Newton as the first inventor of that method, and that Dr. Keill in saying so has done, in their opinion, no injury to Leibnitz.*

The dispute did not and could not well terminate here. Leibnitz complained bitterly of the *Commercium Epistolicum*, and threatened to publish a reply that would confound his antagonists. But he was wise enough not to attempt it. Indeed, it would have been a hopeless and impossible task to have attempted to overturn the evidence, contained in the *Commercium Epistolicum*, of the priority of Newton as the inventor of fluctions. Several anonymous papers were published, either by Leibnitz or his friends, in which Newton was rather attacked than Leibnitz defended. Bernoulli even attempted to prove that Newton did not understand the differential method, as far as the higher orders of fluctions are concerned. At last Leibnitz, associating himself with his friends, the Bernoullis, had recourse to a method which, he conceived, would demonstrate his superiority over the British mathematicians, and thus give his claim of originality a greater chance of obtaining credit. This was to propose difficult problems to embarrass his adversaries. Newton had formerly solved two celebrated problems proposed by John Bernoulli in the year 1697, and an anonymous solution was published that year in the *Philosophical Transactions*. He received the first problem, proposed by the triumvirate, to confound the British mathematicians, after he had undergone a good deal of fatigue in the Mint, and yet he solved it before he went to bed.

During the course of this dispute the extreme partiality which united Bernoulli to Leibnitz induced him to treat Newton with an unbecoming severity, and even injustice. He published, in the *Acta Lipsica*, under a disguised name, a most violent attack upon Keill, in which he endeavours to prove that Newton did not understand the rules of second differenciation. His opinion was founded upon a passage in the *Treatise on the Quadrature of Curves*, in which Newton, from inadvertence, had represented the different orders of fluctions by the terms of his binomial theorem. On the other hand, Keill went too far when he attempted to show that Newton had committed no inadvertence whatever. The dispute at last was silenced, if not terminated, by the death of Leibnitz in 1716.

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* The best edition of the *Commercium Epistolicum* is the octavo of 1722, which contains several articles not to be found in the original quarto edition.

Now that the violence which actuated both parties in this memorable dispute has subsided, and that all party feeling is, in some measure, at an end, we may be allowed to judge of the merits of both parties from the documents that remain. Perhaps the safest way is to reject the arguments brought forward both by the British and German writers of the times, considering them as parties. But there are several French writers who have given their opinions on the subject, and who may be considered as more impartial. The principal of these are Fontenelle, Buffon, Bossut, and Montucla. Fontenelle is very unsatisfactory. He lived too near the time of the dispute, and had, in some measure, committed himself beforehand in his *Eloge* on the Marquis de l'Hospital. Buffon adopts the side of the English mathematicians, and seems to have taken their assertions for granted without ever having examined the documents. Bossut adopts the side of Leibnitz; but it is equally evident that he has never examined the documents, but trusted to the assertions of the Bernoullis, and the other coadjutors of Leibnitz, with whose works he is obviously much better acquainted than with the *Commercium Epistolicum*, and the writings of Newton. His *History of Mathematics* is written with much elegance, and does credit to his taste and talents; but from several particulars which it contains we may infer that it was written when he was a very young man, and that he gives many of the facts which his book contains only at second hand. Montucla gives the most particular and the most impartial account of the dispute of any writer that we have seen; but even he does not appear to have perused all the documents, at least if we may be allowed to give that name to a very elaborate paper which appeared in the *Philosophical Transactions*, giving an account of the *Commercium Epistolicum*, and of the dispute between Leibnitz and Keill. Tradition ascribes this composition to Newton, and there is every reason, from internal evidence, to believe that he was the author.* Now this paper brings forward several striking instances of Leibnitz having attempted to palm Newton's inventions, received in Newton's own writing, upon Newton himself as his own, and of his having desisted from his claim when the trick was pointed out to him. It shows also that Leibnitz had formerly, during the life of Dr. Wallis, acknowledged that Newton was the original inventor of the calculus, and that he had retracted this admission after the death of Dr. Wallis. In short, this paper contains so many important facts, and such accurate reasoning, that it ought to be perused by every person who wishes to form an accurate opinion on the dispute. We think that the following conclusions may be drawn with almost perfect certainty.

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* Phil. Trans. 1714, vol. xxix. p. 173.

1. The method of fluctions, and the differential calculus, are absolutely the same in principle, and differ only in the notation. The reasoning of Newton is rigidly accurate; but that of Leibnitz loose and unsatisfactory. The Leibnitzian notation is conceived to have an advantage over the Newtonian: this advantage, as far as it is real, applies only to the mode of expressing the higher orders of fluctions; and this mode might be introduced, with facility, into the Newtonian notation. But it must be admitted, that the notation introduced by Leibnitz into the integral calculus has advantages, and, accordingly, it has been universally adopted by British writers; which is not the case with his mode of expressing the higher orders of fluctions.

2. Newton was in possession of his method of fluctions many years before Leibnitz thought of his differential calculus, or indeed before Leibnitz had made any great progress in mathematics. This is so obvious from the *Commercium Epistolicum*, that we believe no one will think of calling it in question.

3. Newton more than once announced to Leibnitz that he was in possession of the fluctionary calculus, and points out its advantages with peculiar emphasis; but in none of his letters does he explain the nature of this calculus. There is an explanation of it, however, in Newton's *Analysis per Equationes Numero Terminorum Infinitas*; which was sent up to London by Dr. Barrow, in manuscript. There can be no doubt that Leibnitz saw this paper long before he wrote any thing respecting the differential calculus. The notice, we allow, is very brief; but it is much more than the information given to James Gregory, who, notwithstanding, succeeded in discovering Newton's method.

4. Newton himself admitted, in his *Principia*, that Leibnitz had invented his differential calculus without receiving any information. This passage, which is of great importance to the subject in hand, is as follows:—

"In literas quæ mihi cum geometra peritissimo *G. G. Leibnitio*, annis abhinc decem intercedebant, cum significarem me compotem esse methodi determinandi maximas et minimas, ducendi tangentes, et similia peragendi, quæ in terminis surdis æque ac in rationalibus procederent, et literis transpositis hanc sententiam involventibus (*data æquatione quotcunque fluentes quantitates involvente, fluxiones invenire, et vice versa*) eandem celarem: rescripsit vir clarissimus se quoque in ejusmodi methodum incidisse; et methodum suum communicavit a meo vix abludentem præterquam in verborum et notarum formulis, et idea generationis quantitatum utriusque fundamentum continetur in hoc lemmate."*

This passage appeared in the first edition of the *Principia*, published in 1686; it was continued in the second edition, published in 1713, while the quarrel between Keill and Leibnitz was at the hottest,† but it is not to be found in the edition of 1722. From this passage, there is complete evidence that Newton admitted Leibnitz to have discovered the differential calculus; which he never would have done, if he had furnished him with a sufficient explanation of his own method to enable him to understand it.

5. Newton, then, was the original inventor; Leibnitz knew that he was in possession of some unknown method; this seems to have stimulated his invention. His differential calculus seems to be founded on the method of tangents of Barrow, from which it differs only in the notation. But it was the generalization of that method which constituted the chief merit of Leibnitz. Had Newton published his *Treatise on Fluctions* when it was originally written, he would have had no competitor nor coadjutor, and Leibnitz's mathematical reputation would probably never have risen beyond mediocrity. All concealment in matters of science we consider as improper, and Nature usually punishes it by putting the same invention into the hands of some other person, who deprives the real discoverer of a part of the reputation which he would otherwise have acquired. Newton's hesitation, indeed, proceeded from an amiable and praise-worthy motive; yet it was a weakness, and, as such, was punished by raising up a competitor, who deprived him of his just share of reputation, and gave him more trouble and uneasiness than that which he sought to avoid by withholding his publication.

6. It has been attempted by some to compare Leibnitz to Newton, and to hold him up as scarcely inferior to that illustrious man: but the comparison is very unequal. Leibnitz certainly was a very extraordinary man, and one of the greatest geniuses that ever appeared among mankind: the extent of his

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* Newton's Principia, lib. ii. Scholium, at the end of lemma ii. p. 226, of the edition of 1714.

† Montucla assigns, as a reason why it was continued in that edition, that Cotes, the editor, published it without Newton's knowledge, and against his will. But there must be a mistake in this; for after the preface there is an advertisement, by Newton himself, mentioning a great number of additions and improvements, which he had made in that edition. A proof, not only that he knew of the edition, but that he had been at some pains in correcting and improving it. The paragraph then was retained, because Newton did not wish to erase it. It was doubtless scratched out of the third edition at the suggestion of Keill, or of some other English mathematician engaged in the dispute. Newton's opinion of the dispute is sufficiently evident from his account of the Commercium Epistolicum, in the Phil. Trans.

information was prodigious, his activity was indefatigable, and every region of knowledge which he traversed received marks of his original and inventive genius. But he was far inferior to Newton, both as a philosopher and as a man. None of his productions will bear a comparison with the *Principia*, or the *Optics*, of Sir Isaac Newton; nor do we think any of his mathematical writings equal to the *Universal Arithmetic*, or the *Fluctions of Newton*. We cannot conclude this account better than by giving the comparison between the two men, drawn up by Newton himself, on occasion of this very controversy.

" It must be allowed that these two gentlemen differ very much in philosophy. The one proceeds on the evidence arising from experiments and phenomena, and stops where such evidence is wanting; the other is taken up with hypotheses, and propounds them, not to be examined by experiments, but to be believed without examination. The one, for want of experiments to decide the question, does not affirm whether the cause of gravity be mechanical or not mechanical: the other, that it is a perpetual miracle, if it be not mechanical. The one, by way of inquiry, attributes it to the power of the Creator, that the least particles of matter are hard; the other, attributes the hardness of matter to conspiring notions, and calls it a perpetual miracle, if the cause of this hardness be other than mechanical. The one does not affirm that animal motion in man is purely mechanical: the other teaches that it is purely mechanical, the soul or mind (according to the hypothesis of a harmonia præstabilita) never acting on the body so as to alter or influence its motions. The one teaches that God (the God in whom we live, and move, and have our being,) is omnipresent; but not a soul of the world: the other, that he is not the soul of the world, but intelligentia supra mundana, an intelligence above the bounds of the world; whence it seems to follow, that he cannot do any thing within the bounds of the world, unless by an incredible miracle. The one teaches, that philosophers are to argue from phenomena and experiments to the causes thereof, and thence to the causes of those causes, and so on till we come to the first cause: the other, that all the actions of the first cause are miracles, and all the laws impressed on nature by the will of God are perpetual miracles and occult qualities, and, therefore, not to be considered in philosophy. But, must the constant and universal laws of nature, if derived from the power of God, or the action of a cause not yet known to us, be called miracles and occult qualities, that is to say, wonders and absurdities? Must all the arguments for a God, taken from the phenomena of nature, be exploded by new hard names? And must experimental philosophy be exploded as miraculous and absurd, because it asserts nothing more than can be proved by experiments, and we cannot yet prove by experiments, that all the phenomena in nature can be solved by mere mechanical causes? Certainly these things deserve to be better considered."*

Article II.

A Graphical Representation of the daily Rate of the Barometer during a Year in London, Paris, and Geneva. By J. P. Pictet, Assistant Professor of Natural Philosophy in Geneva.†

The representation in Plate XIV. was made out by the author upon a very small scale from a great collection of observations of the rate of the barometer at London, Paris, Geneva, and Madrid, laid before the Natural History Society of Geneva. It contains a complete year of observations, made in the three first named cities. The upper half of the copper-plate reaches from the autumnal equinox of 1806 to the vernal equinox of 1807, the lower half extends from this equinox to the autumnal equinox of the same year. "We see here very evidently," says M. Pictet, "how much more the barometer rises and falls in winter than in summer. The complete harmony which exists between the three barometrical curves, and the very regular parallelism of their inflections is surprising, when we consider the great distance of the three places from each other. ‡

In January and February we find the greatest, in July and August the smallest alterations in the height of the barometer. Very high and very low positions correspond in all the three places within a day. The figures upon the plate mark the day of the month on which these *maxima* and *minima* were observed. An exception is observable in October, 1806. The barometer was highest in London on the 24th, at Paris on the 25th, and at Geneva on the 26th. Likewise in January, 1807, it was highest at London on the 5th, in Paris on the 6th, and at

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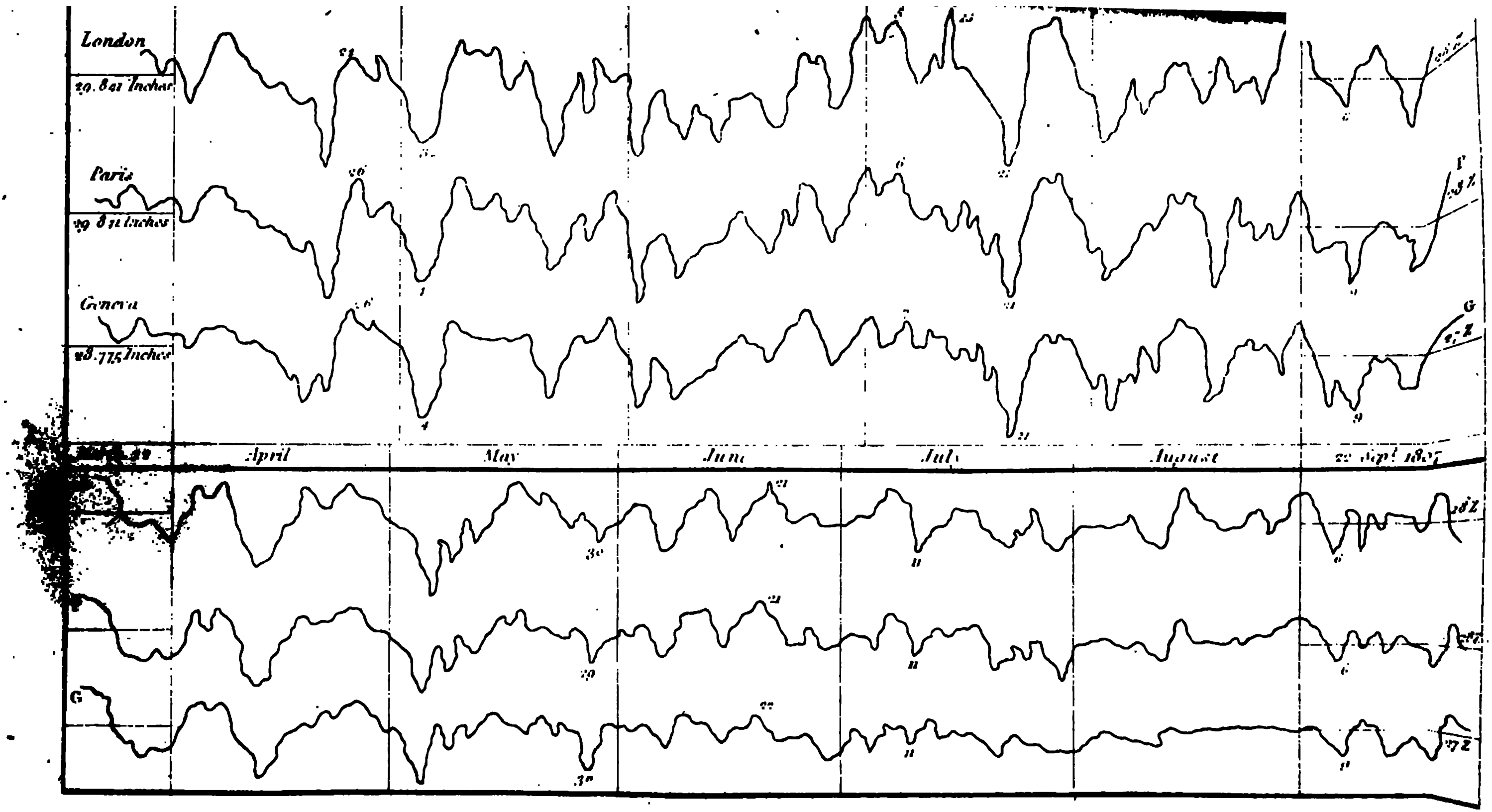
* Phil. Trans, 1714, vol. xxix. p. 173.

† This curious paper was published in the *Bibliothèque Britannique* for Jan. 1811. I have translated it from *Gilbert's Annalen* for May, 1812. It is evident that the German editor has made considerable alterations in the original.—T.

‡ The same thing holds good in another set of barometrical curves constructed by M. Pictet upon a much larger scale, embracing a longer period of time, and including likewise a curve for Madrid.

When these curves are compared, it appears that all the principal inflexions agree very nearly with each other in time and amount; only, the farther north a place lies, the sooner does the change begin at it.


London
29.841 Inches
Paris
29.811 Inches
Geneva
28.775 Inches
April
May
June
July
August
22 Sept 1827
G

Geneva on the 7th. In November the barometer stood lowest in London on the 3d, in Paris and Geneva on the 4th. As these alterations in the weight of the atmosphere begin usually at London, they seem to proceed chiefly in a direction from west to east; but this commencement in London is not without exception. In May, 1807, the barometer was lowest at Paris on the 29th, at London and Geneva on the 30th.

Though the curves have a great resemblance to one another upon the whole, yet there are some particulars in which they differ: for example, in London the barometer sunk without interruption from the 13th to the 21st of January, 1807; while in Paris and Geneva it made several oscillations during the same time, which agree with each other in these two cities. On the 13th of January, 1807, the barometer sank slowly in Paris and Geneva; but in London it descended with such velocity that in 48 hours it had sunk 0·71 inch. The cause of this change in the barometer must be very powerful, yet completely local, and of very short duration, since the rate of the barometer agrees within a day in London, Paris, and Geneva. It were to be wished that we knew the state of the atmosphere at London on the day of this rapid descent of the barometer. This knowledge would probably throw some light on so sudden a fall.*

I consider the present attempt to be sufficient to show how important it would be to keep daily tables of meteorological observations on different parts of the meridian and parallel of Paris, and to exhibit these observations every year in the state of curves. By this method more knowledge of the modifications which take place in the atmosphere would be gained by a single glance of the eye than by a painful comparison of voluminous observations. To save trouble, tables might be published so contrived that the observer would have no more to do than to denote the daily observation by a point upon the table. At the end of every half year the curve may be directly drawn. A copy of such a graphic delineation might be sent to the central board, which already exists under the name of Board of Longitude, and holds its meetings at the observatory of Paris. Here a complete table of all the observations would be constructed, and a copy of it sent to every observer; from which he would see the fruit of his labour, and be induced to continue his observations. The choice of the places of observation, accurate directions for the benefit of the observers, the precision of the instruments, and the general result of the comparison, are particulars which obviously belong to the consideration of the learned in this celebrated society; and they appear highly worthy of their attention and their zeal.

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* In Great Britain it holds, without exception, as far as my observations have gone, that when the barometer falls suddenly a violent wind soon follows, not always at the very place of the fall, but within no very great distance. I have no doubt that the fall alluded to by M. Pictet was followed by a violent south-wester.—T.

Article III.

On the Composition of Oxide of Zinc. By Thomas Thomson, M.D. F.R.S.

In the table of the proportions in which chemical bodies unite, published in the *Annals of Philosophy*, vol. ii. p. 42, I have stated that the oxide of zinc is composed of 100 metal + 23·175 oxygen. This number is the mean of experiments made to determine the point by Berzelius, Davy, and myself. But as the zinc of commerce, with which my experiments, and those of Sir Humphry Davy, had been made, is always contaminated with some lead, and not unfrequently also with copper, it is obvious that experiments to determine the composition of the oxide of zinc by directly oxidating that metal cannot be perfectly accurate, and will probably give the proportion of oxygen below the truth. This consideration induced me to make a set of experiments in a different way, and susceptible of greater accuracy, in order to see how far the proportions which I had stated in the table deviated from the truth. I shall here give the result of my experiments, for the satisfaction of my readers.

1. The sulphuric acid which I had in my possession was of the specific gravity 1·83993, at the temperature of 60°. My first object was to ascertain the proportions of real acid and water contained in this liquid: 100·4 grains of the acid were diluted with water, neutralized by a base, and precipitated by muriate of barytes. The sulphate of barytes obtained was washed, dried, and exposed to a red heat. It weighed 220·1 grains. The experiment being repeated, the sulphate of barytes obtained weighed 228·78 grains. A third experiment was made with 100·06 grains of the acid. The sulphate of barytes obtained weighed 220·67 grains. These experiments were all made with equal care; and the want of agreement between them must be ascribed to the imperfection of our methods of experimenting, and not to any want of attention on my part. If we consider sulphate of barytes as composed of 100 acid + 194 base, according to the experiments of Berzelius, then the sulphuric acid is composed

By 1st experiment, of	74·564 acid	+	15·436 water
2d	75·813	+	14·187
3d	74·910	+	15·090
Mean......	75·096	+	14·904

I may remark here, that this result almost exactly coincides with the table of the strength of sulphuric acid published by Mr. Dalton (*New System of Chemistry*, part ii. p. 404); for by that table we see that acid of the specific gravity 1·838 contains 75 per cent. of real sulphuric acid. Now the acid which I used was of the specific gravity 1·8399, and contained 75·096 per cent. of real acid.

2. I diluted 100·4 grains of this acid with water, and put into it a plate of zinc. The acid was allowed to act upon the zinc for 24 hours. It then had lost the power of reddening paper stained with litmus. Hence I considered it as neutralized. The quantity of zinc dissolved was 62·7 grains. The experiment was repeated with the same weight of acid, and the zinc was allowed to remain in the liquid for two days. The quantity dissolved was 62·93 grains. Supposing it possible that some portion of the acid might remain uncombined with the oxide of zinc for want of time, I repeated the experiment a third time: 100·06 grains of acid were diluted with water, and a plate of zinc of considerable size was kept in it for nine days, the liquid being frequently agitated, and the zinc brought in contact with every part of it. The portion of zinc dissolved in this case was only 61·5739 grains.

According to these experiments, 100 grains of real sulphuric acid dissolve

By 1st experiment	83·168 grains of zinc
2d	83·465
3d	81·944
Mean	82·859

3. I consider it as established by experiment that 100 parts of pure sulphuric acid are neutralized by a quantity of base, which contains 20·02 of oxygen. Thus, for example, 100 sulphuric acid combine with 280 of yellow oxide of lead; and 280 of this oxide contain 20·02 of oxygen. So, likewise, 100 of sulphuric acid combine with 100 of black oxide of copper, which contain 20 of oxygen. It follows from this, since zinc has the property of precipitating copper from sulphuric acid, and taking its place, that the 82·859 of zinc which unite with 100 of sulphuric acid must previously combine with 20·02 of oxygen. Therefore 100 sulphuric acid combine with 102·879 of oxide of zinc; and the

oxide of zinc is composed of 82·859 metal + 20·02 oxygen, or of 100 metal + 24·16 oxygen.

The preceding experiments warrant us, I conceive, to consider the oxide of zinc as composed of 100 metal + 24·16 oxygen. Now this determination approaches much nearer to that of Berzelius, who found oxide of zinc composed of 100 metal + 24·4 oxygen; than to that of Sir H. Davy, who makes it 100 metal + 21·951 oxygen; or myself, who obtained the numbers .00 metal + 23·5 oxygen, by direct experiment. This new determination of the composition of the oxide of zinc makes it necessary to alter the numbers representing the weight of an atom of zinc in the table (*Annals of Philosophy*, vol. ii. p. 49). Instead of 4·315, the corrected number will be 4·139, and the weight of the oxide of zinc will be 5·139.

These new numbers do not agree quite so well with my analysis of the sulphate of zinc (stated in the *Annals of Philosophy*, vol. ii. p. 297,) as the preceding ones, which I employed in my table. I found sulphate of zinc composed of 25·8 acid + 28·2 oxide. Now 5 (the weight of an atom of acid) : 5·139 (the weight of an atom of oxide) :: 25·8 : 26·517. But I find, upon considering my analysis, that the quantity of sulphuric acid obtained is underrated. 100 grains of crystallized sulphate of zinc yielded 78·2 grains of sulphate of barytes. Now 78·2 of sulphate of barytes contain, according to the analysis of Berzelius, 26·6 of sulphuric acid. Now 5 : 5·139 :: 26·6 : 27·34. This does not exactly coincide with the 28·2 of oxide of zinc obtained. But I find two circumstances stated in the account of my analysis which appear to me quite sufficient to account for the difference. 1. The sulphate of zinc which I analysed is stated to redden vegetable blues. Now I find that the salt obtained by dissolving zinc in sulphuric acid has not that property. Hence the salt analysed must have contained a slight excess of acid. 2. The precipitate of oxide of zinc is described as flesh coloured. This shows that it was mixed with iron, and consequently that the salt was not quite pure.

Article IV.

Account of a New Hydraulic Machine. Contrived by M. Mannoury Dectot.*

The Class has already approved of several machines invented by M. Mannoury. The one which we have at present to describe is not less worthy of attention.

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* This is a translation of the Report made to the Institute on the subject on the 23d August, 1813. I have translated it from the Moniteur of the 27th August.

M. Mannoury not having yet given a name to his new machine, we shall venture to call it a *danaide*, as this name seems proper to give a first conception of the mechanism of the machine, which in fact is nothing else than a trough into which the water is continually running from above, while as great a quantity runs out by a hole at the bottom. But the *danaide* of M. Mannoury is more fortunate than that of the daughters of the King of Argos; because it is more ingenious. He has contrived to give his trough a rotatory motion, to prevent by its centrifugal force the too great rapidity of the escape of the water: so that if the secrets of M. Mannoury should ever make their way into Tartarus, he will have the glory of alleviating the labours imposed for so many ages upon these unfortunate princesses.

The model, by means of which M. Mannoury exhibited his experiments to us, consists principally, as has been said, of a trough, the bottom of which has a hole in its centre. This trough is cylindrical, and nearly as high as it is broad, and is made of tin plate. It is fixed to a vertical axis of iron, which passes through the middle of the hole in the bottom, leaving a vacant space all around, through which the water escapes as it flows into the trough. This axis turns with the trough upon a pivot, and is fixed above to a collar.

The object of M. Mannoury was, that the water flowing into the trough from above with a certain quantity of *vis viva*, should communicate the whole of it to the solid parts of the machine, to be employed afterwards in producing some useful effect; always excepting the small quantity of force necessary to enable the water to escape by the orifice below. He obtains his object in the following manner.

To the axis of the trough, and within it, there is fixed a drum, likewise of tin plate, concentric with the trough, and close above and below. This drum, which turns round with the trough, fills almost the whole of its solid contents: there is merely a small space between the two, not exceeding 1·5 inch. This space exists likewise between the bottom of the trough and the drum, but it is in that place less, and is divided into several compartments by diaphragms proceeding from the circumference to the central hole in the bottom of the trough. These diaphragms do not exist between the sides of the drum and the trough, and the compartments at the bottom communicate with this annular space.

The water comes from a reservoir above by one or two pipes, and makes its way into this annular space between the trough and drum. The bottom of these pipes corresponds with the level of the water in the trough, and they are directed horizontally, and as tangents, to the mean circumference, between that of the trough and of the drum. The velocity which the water has acquired by its fall along these pipes makes the machine move round its axis; and this motion accelerates, by degrees, till the velocity of the water in the space between the trough and drum equals that of the water from the reservoir: so that no sensible shock is perceived of the affluent water upon that which is contained in the machine.

This circular motion communicates to the water between the trough and drum a centrifugal force, in consequence of which it presses against the sides of the trough. This centrifugal force acts equally upon the water contained in the compartments at the bottom of the trough, but it acts less and less as this water approaches the centre.

The whole water, then, is animated by two forces, which oppose each other; namely, gravity and the centrifugal force. The first tends to make the water run out at the hole at the bottom of the trough; the second tends to drive the water from that hole.

To these two forces are joined a third, namely, *friction*, which acts here an important and singular part, as it promotes the efficacy of the machine, while in other machines it always diminishes that efficacy. Here, on the contrary, the effect would be nothing, were it not for the friction, which acts in a tangent to the sides of the trough and the drum.

By the combination of these three forces, there ought to result a more or less rapid flow from the hole at the bottom of the trough: and the less force the water has as it issues out, the more it will have employed in moving the machine, and of course in producing the useful effect for which it is destined.

The moving power is the weight of the water running in, multiplied by the height of the reservoir from which it flows above the bottom of the trough: and the useful effect is, the same product, diminished by half the force which the water retains when it issues out of the orifice below.

We endeavoured to ascertain by a direct experiment the amount of this useful effect. We fixed a cord to the axis of the machine, which by means of pulleys properly placed raised a weight in proportion as the machine turned round. The result of repeated experiments was, that the effect produced amounted to $\frac{7}{10}$ of the moving cause, and often approached $\frac{75}{100}$, even not reckoning the friction of the pulleys, &c. which has nothing to do with the machine. This effect surpasses considerably that of the best machines known.

Hence this new piece of mechanism, notwithstanding its great simplicity, ought to be placed among the most important conceptions for the arts, and we propose to the Class to give it its approbation.

(Signed) **Perier, Prony, Carnot,** *Reporters.*

The Class approves of the Report, and adopts its conclusions.

Delambre, *Perpetual Secretary.*

Article V.

General Views of the Composition of Animal Fluids. By J. Berzelius, M.D. Professor of Chemistry in the College of Medicine at Stockholm.

(*Concluded from p.* 387.)

Of the Fluids that compose the Excretions.

I must refer the reader to what I have already observed generally on this subject, and shall proceed to the individual species.

1. *The Fluid of Perspiration.*

I have not been able to make my experiments on this fluid in any large quantity, the organ that secretes it being extended over so large a surface, and seldom affording much at a time. As to what collects in under-waistcoats after some days wearing, it must always be more or less altered by spontaneous decomposition.

I collected on a watch-glass a few drops of sweat as they fell from my face, and evaporated them carefully. The yellowish residue had all the appearance under the microscope of the usual mixture of the muriates of potash and soda with lactic acid, lactate of soda, and its accompanying animal matter. It reddened litmus, and dissolved in alcohol, and was without doubt of the same nature as the analogous matter found in the other fluids. The alcohol left untouched a small trace of an animal matter which blackened in the fire, but was in too small quantity to admit of further examination.

Mr. Thenard discovers acetous acid in perspiration, but this acetous acid is produced here, as in his other experiments, from the lactic, by his mode of operating. It is well known that litmus paper reddens instantly when put in contact with the skin of the living body; which shows that the acid that produces this effect is not volatile, otherwise it would be evaporated by the dry surface of the body, which has always a temperature of 86° to 90° Fahr.

2. *Urine.*

We possess many analyses of urine, both in a healthy and diseased state, but none of them gives a very extended view of the subject; and it is clear that the analysis of morbid urine acquires its chief interest from being able to compare it with that of health.

A. *The Acids of the Urine.*—The acidity of healthy urine has generally been attributed to the phosphoric acid. By the chemical change which the blood undergoes in the kidnies, a large portion of its constituent parts are acidified; so that the blood which enters alkaline into the renal arteries, returns from the renal vessels loaded with many acids, some of which did not at all exist in the blood at its entrance, and others were present in very minute quantity only. The acids of the urine which do not at all exist in the blood are the sulphuric, uric, and sometimes the benzoic; the others are the phosphoric and lactic. The muriatic and fluoric acids appear to pass from the blood to the urine without increase in their proportional quantity. As by the laws of chemical affinity these acids will unite with any alkali that may be present, and saturate themselves with it in the order of the force of their respective affinities, it must follow that where the quantity of alkali is insufficient to saturate all the acids present, the weakest acids must be those that will remain uncombined and will give the urine its acid properties. These therefore must be the lactic and the uric.

It is so generally known and so fully proved that the urine contains the *phosphoric*, *muriatic*, and *uric* acids, that it is useless to add any thing further on this subject.

Urine contains the *fluoric* acid. In my analysis of bones I have found that human and ox bone contain as much as two per cent. of fluate of lime. It is therefore natural to suppose that the earthy phosphates dissolved in urine, which are chiefly derived from the decomposition and absorption of bone, should also retain the same proportion of fluate of lime. To prove it, I precipitated a large quantity of urine with caustic ammonia, collected and calcined the precipitate, mixed an ounce of it with as much sulphuric acid, and then heated the mixture moderately in a platina crucible covered with a glass plate prepared for etching. After some hours I removed the glass, took off the graver's wax, and found the lines corroded by the fluoric acid vapour.

Urine saturated by ammonia, filtered, and mixed with muriate of lime, gives also a good deal of phosphate of lime, containing no fluoric acid. The urine therefore contains no other fluate than that of lime.

The *sulphuric* acid is also found in urine. The alkaline fluids merely neutralized by acetic or muriatic acid, and then mixed with muriate of barytes, give no indication of sulphuric acid. But if the residue left after the evaporation of these fluids be first calcined, and the saline part then extracted by lixiviating the ash, and treated with muriatic acid and muriate of barytes, a notable quantity of sulphuric acid is found, produced from the sulphur contained in animal matter. But with urine the appearances are very different. Muriate of barytes added to it forms immediately a copious precipitate of sulphate of barytes; and I have constantly found that the quantity of sulphuric acid in urine exceeds that of the phosphoric acid. Rouelle, the elder, long ago detected sulphuric acid in urine, but this was considered as an accidental circumstance: I have however very good reason for supposing this acid to be a most essential constituent part of urine. The production of this acid takes place in the kidnies, and the action of these organs resembles combustion in this respect, that part of the constituent elements of the urine, such as the sulphur, phosphorus, the radicals of the alkalies and earths, &c. become oxidated to their *maximum*: and here also the kidnies generate some acids with compound bases. The remainder of the blood gives rise to the urea, which, being formed from the materials of the blood, ought to contain more azote in proportion as a greater number of the other elements of these materials has been acidified. It would however be incorrect to consider the production of urine as an excretion of the azote which is in excess in the animal economy, because it appears that the amount of its quantity in the constituents of urine is not greater than in the constituents of blood. We might with as much reason consider the kidney as an organ for oxidation; but it is certainly an error to believe that any organ except the lungs has the office of removing one particular element in a greater proportion than the rest.

I at first conceived that the whole of the sulphur contained in the blood was acidified in the kidnies, and consequently imagined that the same change might be induced on a part of the azote, the carbon, and the hydrogen. Proust asserts that the urine contains carbonic acid; but it is very difficult to establish the certainty of this fact, because the urea is decomposed by a heat even inferior to that of boiling water, and produces carbonate of ammonia, which is decomposed by the free acid of the urine, and carbonic acid is disengaged. I am rather disposed to believe that urine does not contain carbonic acid, because no bubbles of air are ever seen to form on the sides of the containing vessels: and if urine still warm be thrown on any substance whatever reduced to powder, such as powdered sugar, no effervescence ensues. Urine is found to contain no nitric acid. If the residuum from evaporation be treated with alcohol, and the undissolved portion (which must contain all the nitrates of the urine) be exposed to fire, it will exhibit no sign of detonation: although this phenomenon is very perceptible whenever a very small quantity of nitrate of potass, or even of nitric acid, has been added to the urine. Lastly, I found accidentally that the whole of the sulphur contained in urine is not acidified. I had made use of nitrate of barytes to precipitate the sulphuric acid in urine, and had previously added to the latter some nitric acid, in order that the phosphate of barytes might be retained in solution. After having separated the sulphate of barytes, I precipitated the phosphate by ammonia; and after having filtered the ammoniacal liquor, I evaporated it. During its evaporation it deposited small white shining crystals, exceedingly hard, insoluble in water, in acids, or in caustic potass. After subjecting them to many experiments, I at length found them to be sulphate of barytes. In repeating the experiment with muriate of barytes and muriatic acid, in order to prevent the precipitation of the phosphate of barytes, no sulphate of barytes was formed. The production of this salt is the more singular because the nitric acid present was even supersaturated with ammonia. Instructed by this experiment, I precipitated another portion of urine with nitrate of barytes, and after filtering evaporated it to dryness, and burned the residuum with a fresh quantity of nitrate of barytes. The ashes treated with muriatic acid left a considerable quantity of undissolved sulphate of barytes.

4. *The benzoic acid* is found, according to Scheele, in the urine of infants. I have not been able, in my experiments, to discover the least trace of it, and I much doubt whether it be contained in acidulous urine.

5. *Lactic acid.*—It is principally to this acid that urine owes its acid properties: and if I may be allowed to speculate on final causes, I should say that it is destined to hold the earthy phosphates in solution, and obviate the dire effects of their deposition in a solid mass. In order to ascertain the presence of this acid, the urine must be evaporated to the consistence of a syrup, and treated with alcohol. The substance which remains undissolved is acid, and by the addition of ammonia is decomposed; and the lactic acid combined with the ammonia becomes soluble in alcohol. From its solution in alcohol the ammonia is disengaged by quicklime, and from the new salt thus formed the lime may be separated by oxalic acid, which leaves the lactic acid dissolved in water. By this process a small part only of the lactic acid contained in the urine is obtained, the greater part, together with the lactate of ammonia, being carried off by the alcohol.

B. *The Deposit from Urine.*

Urine, by cooling, affords a deposit which varies considerably in different circumstances, not only in quantity, but also in external characters. When it is abundant the urine becomes turbid throughout, a grey powder is precipitated, and after continuing for some time at rest, the precipitate is found at the bottom covered with a mucous stratum. The deposit gradually acquires a red tinge, and after some time is found perfectly crystallized. When the urine does not become turbid, there only appears a thin cloud scarcely discernible, which by rest sinks to the bottom or collects in light transparent flocculi, in which there are sometimes formed, after 24 hours, red crystals.

All urine, when newly evacuated, contains a matter suspended in it, which in some degree affects its perfect transparency. This matter is the mucus of the inner coat of the bladder. If the urine, while yet warm, be poured on a filter, it will pass perfectly clear, and the mucus will remain on the filter in the form of transparent and colourless flocculi. The deposit which afterwards takes place in filtered urine is pulverulent, and nowise mucous; which proves that it is from admixture with the mucus of the bladder, that the flocculent appearance, so frequently assumed by the deposit, arises. By drying, the mucus loses its transparency, becomes red, and sometimes has a crystalline aspect, which is owing to the presence of uric acid, the crystals of which have a diameter exceeding the thickness of the dried mucus.

If a person, after continuing for a long time either in a standing or sitting posture, without much exercising his muscles, should evacuate urine successively in different vessels, the first portions will contain the largest quantity of mucus, the next less, and the succeeding portions none whatever. This arises from the mucus being heavier than the urine, and collecting in the lower part of the reservoir; but if, on the other hand, a person be obliged to remain long on his back, and to evacuate the urine in that position, the lowermost portion does not come away first, and it then frequently happens that the whole of the mucus cannot be evacuated, and that a part remains for a long time in the bladder, when, from the disposition of the uric acid to form crystals in the mass of mucus,* the latter becomes a nucleus for the formation of calculus. To this circumstance may be ascribed the frequency of this disorder originating during the cure of fractures of the lower extremities.

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* I have very often observed that when the urine has been evacuated in two portions, of which the one contained mucus, and the other was clear, the latter has afforded no precipitate, while in the former the mucus was found after 12 or 24 hours, to contain numerous red crystals, sometimes of considerable size. It would therefore appear that in urine, which does not contain enough of uric acid to form a precipitate by cooling, the presence of mucus favours in some particular way its crystallization.

The cloud which appears in the urine during fever, is in fact merely the mucus of the bladder, which, from the increase of the specific gravity of the urine, subsides more slowly, or sometimes remains suspended in the fluid. The deposition which urine exhibits in diseases frequently suggests useful indications, and its examination is sometimes of great importance in the practice of medicine. It is necessary, in this inquiry, to distinguish between two different kinds of matter deposited: the one being composed of the materials which are not held in solution, and are mechanically suspended, and the other consisting of those which are dissolved in warm urine, but which separate by cooling. The former contains, in a state of health, only the mucus of the inner coat of the bladder; which, while yet warm, may be separated from the latter by filtration. The mucus that remains forms discrete flocculi, which do not collect together, and which, after being dried, do not recover their original transparency and mucosity by maceration in water. This mucus is in great part dissolved by acetic acid or diluted muriatic acid; but diluted sulphuric acid acts upon it very feebly. It is also soluble by digestion in caustic alkalies.

In catarrhus vesicæ the urine is loaded with an enormous quantity of a mucous matter which is suspended in it. This matter is a true mucus, although, in consequence of the morbid affection of the organ which produces it, its characters are different from those of healthy mucus. If it be collected on the filter, in proportion as the water is absorbed, it becomes more and more mucous and viscous; and, during the desiccation, it becomes transparent and greenish. By maceration in water it recovers its mucous character, undergoes after some time an acid fermentation, and acquires a purulent appearance. In a word, the mucus of the bladder, when diseased, approaches more nearly to that of the nose, and differs much in its properties from the secretion in its natural state.

There is still another morbid condition of the urinary passages, in which the urine carries along a matter mechanically suspended in it, and which has so close a resemblance to that produced by catarrh, that inattentive practitioners easily confound the one with the other. The urine when filtered leaves a mucous matter on the filter, which however does not become transparent by desiccation, but gives, on the contrary, a white powder appearing only to the touch. This powder consists of phosphate of lime, and the ammoniaco-magnesian phosphate, mixed with the mucus of the bladder. The urine in this disease has lost all its free acid, it does not affect the colour of litmus paper, and I have sometimes even seen it restore the blue colour of litmus when it had been reddened by vinegar. In observing this re-action, the paper should be inspected immediately; if it be allowed to dry, it reddens from the decomposition of the ammoniacal salts, and this happens even when the paper is immersed in a solution of neutral muriate of ammonia, or even when the ammonia is in excess. The urine in this disease has also the property of yielding a precipitate by common muriate of mercury, in the same way as during a paroxysm of fever: a circumstance which is owing to the absence of the free acid.*

The secondary precipitate which is formed in urine, which has been filtered while still warm, is pulverulent, and contains, as is already known, the uric acid in abundance. In the first moments after its formation it is of a greyish white, but it gradually acquires a reddish hue, and the pulverulent matter assumes at the same time a crystalline form. The change is effected still more quickly when exposed to the immediate contact of air, than when the deposit is covered with urine. The greyish precipitate which is first formed is soluble in caustic alkali, without the evolution of ammonia; but in proportion as it becomes red and crystallized, potass disengages from it ammonia in abundance. It is evident, therefore, that the crystallization of the precipitate depends on the formation of urate of ammonia with excess of acid, which appears to constitute the red crystals which form in urine by cooling. I think I have observed that the mucus is in a great measure concerned in this change in the precipitate, since it takes place more slowly, and in a less degree, in urine which has been filtered. That species of urine which on cooling becomes milky, and appears like a mixture of clay and water, yields about one-thousandth of its weight of precipitate.

The deposit, treated with acetic acid, is partly dissolved, and gives a yellowish solution, from which a precipitate is thrown down by carbonate or prussiate of potass, as well as by infusion of galls, but not by the caustic alkalies. The quantity dissolved is greater, and the yellow colour more intense, when the urine had not been filtered before cooling; which appears to prove that the substance dissolved by the acetic acid is in both cases mucus, of which a part has been dissolved in the urine, and since precipitated in a state of chemical combination with the uric acid. It is this compound which is gradually decomposed, and gives rise to the crystallized superurate of ammonia. The deposit contains none of the earthy phosphates. The muriatic acid digested with the deposit, and then saturated with ammonia, precipitates nothing. Subjected to fire, the deposit burns, and leaves at length, and with some difficulty, a very small quantity of a fused ash, which consists of carbonate of soda, proving that the deposit often contains a small quantity of superurate of soda, that insoluble salt which, according to the experiments of Dr. Wollaston, produces the gouty concretions of the joints.

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* I once had occasion to treat a man attacked with this disease. I endeavoured by large doses of phosphoric acid to supply the deficient acid in the urine, but without being able to effect any alteration whatever. The dose was at length increased till it purged the patient, when the urine suddenly resumed its natural characters, and became acid, transparent, and deposited uric acid. But these salutary effects disappeared with the purgative one, and they could not be again reproduced. After the phosphoric acid had been employed in vain, the acetic and sulphuric acids were given, but without success. Alkaline remedies had no effect either beneficial or injurious, and the disease still continues, having produced a great degree of debility in the inferior extremities.

The secondary deposit of healthy urine, therefore, is not uric acid, but a combination of this acid with an animal matter, which appears to be a portion of the mucus of the bladder dissolved by the warm urine. The deposit contains still a trace of superurate of soda, and by spontaneous decomposition there is formed superurate of ammonia, which renders it crystalline.

It is to be presumed that uric acid, which is deposited in the bladder and forms calculi, contains this same animal matter, which ought accordingly to be an essential constituent of all calculi formed in the bladder. I have thus found it in two different calculi which I examined with this view. The following is the mode of separating the uric acid from the animal matter. The calculus is dissolved in caustic alkali, and a precipitate obtained by adding to the solution muriatic acid in excess. The precipitate consists of uric acid; and a combination of the animal matter with muriatic acid, which may be carried off by washing the precipitate freely on the filter. The muriatic compound is soluble in pure water, and is again precipitated by the addition of muriatic acid, or by allowing it to drop into the acid liquor which has passed through the filter. The uric acid remaining on the filter is in a state of purity greater than any that has been hitherto the subject of examination.

C. *Analysis of Urine.*

I have been at much pains to arrive at as accurate a knowledge as possible of the precise composition of urine, both as to the quantity and condition of its constituents. The task has been laborious, difficult, and often extremely complicated. It would be trespassing on the patience of the Society, were I to attempt at present to give an account of all the details, and such is the nature of an exact analytical investigation that it admits not of a brief recital. I shall therefore content myself with communicating the general result, which is as follows:—

1000 parts of urine are composed of

Water	933·00
Urea	30·10
Sulphate of potass	3·71
Sulphate of soda	3·16
Phosphate of soda	2·94
Muriate of soda	4·45
Phosphate of ammonia	1·65
Muriate of ammonia	1·50
Free lactic acid Lactate of ammonia Animal matter soluble in alcohol, and usually accompanying the lactates Animal matter insoluble in alcohol Urea not separable from the preceding	17·14
Earthy phosphates with a trace of fluate of lime	1·00
Uric acid	1·00
Mucus of the bladder	0·32
Silex	0·03
	1000·00

With regard to the relative proportions of these ingredients, it is very probable they may vary independently of disease. I believe, however, that in urine they are never very different, unless from pathological causes, which materially affect the health.

I should also observe that in the 17·14 parts of lactic acid, lactate of ammonia, &c. there exists a quantity of water which it is not possible to abstract without the risk of decomposing these substances. The quantity of uric acid always varies according to the individual, and also in the same individual from different circumstances which have little influence on health. In the above analysis it was determined from urine which became turbid throughout during its cooling, and which during its deposition resembled water mixed with clay.

The earthy phosphates contain much more magnesia (as much as 11 per cent.) than in the bones or the ashes of blood. Of the cause of this I am ignorant; but I have likewise found much more potass in urine and in milk than in the blood.

The silex was not detected by the composition of dried urine: for in this way it might have been considered as a constituent of some animal matter dissolved in the urine. I discovered it by treating evaporated urine with alcohol, then with water, and afterwards with muriatic acid, which left the undissolved silex in the form of a grey powder, which fused with soda produced a transparent glass, and which by the decomposition of the glass was converted into the gelatinous state. The water we drink, and which supplies the continual waste of that element by perspiration and by urine, always contains silex, which appears not to separate from it in the body, and which thus makes its exit in the same state in which it entered. It is evident that this earth should be found dissolved in the other animal fluids, and that the quantity must vary according to the quantity contained in the water used as beverage.

Milk.

My experiments have chiefly been made on the milk of the cow. The composition of this fluid is exceedingly analogous to that of blood. It consists, like the blood, of a chemical solution, and an admixture of undissolved matter suspended in it. By exposing milk for some days in a shallow vessel to the temperature of 32° Fahr. I separated from it the cream as completely as I could. The lower portion of milk, decanted by a hole at the bottom of the vessel, had a specific gravity of 1·033, and yielded by analysis the following constituents:—

Water	928·75
Cheese, with a trace of butter	28·00
Sugar of milk	35·00
Muriate of potash	1·70
Phosphate of potash	0·25
Lactic acid, acetate of potash, with a trace of lactate of iron	6·00
Earthy phosphates	0·30
	1000·00

Cream contains the emulsive matter which is not dissolved, more concentrated and mixed with a portion of milk. This emulsion is easily decomposed by agitation, absorbs oxygen, and the butter separates: the milk becoming by this operation more acid than it was at first. I found that cream of the specific gravity 1·0244 was composed of

Butter	4·5
Cheese	3·5
Whey	92·0

As 92 parts whey contain 4·4 sugar of milk and salts, it follows that cream contains about 12·5 per cent. of solid matter.

It is very remarkable that scarcely any other alkali than potash is found in milk. I have burnt a quantity of dried milk, and have dissolved the muriate of the ashes in spirit of wine; and the alkali left undissolved by the alcohol, neutralized by the sulphuric acid, produced only sulphate of potash. I know not how far this observation is applicable to other kinds of milk, or to milk taken from other individuals.

Cheese, which is destined to be part of the nourishment of the young animal, has very peculiar characters, which, as it would seem, fit it for this office. It admits easily of incineration, affording a white ash which contains no alkali, and which forms as much as 6·5 per cent. of the weight of the cheese. This ash contains principally earthy phosphates with a little pure lime: but it contains neither alkali nor oxide of iron. Cheese digested with concentrated muriatic acid yields the greater part of its phosphates to the acid, and it afterwards burns without leaving any ash. But the cheese may be precipitated from the milk by an acid without losing its phosphates. It appears, then, that the latter are not yet formed, but that a slight affinity only is requisite to their production. We may conclude that nature has thus sought to assist the digestive powers of the young during a period of their lives in which there exists in the economy the greatest demand for earthy phosphates for the purposes of ossification, which is at that time advancing so rapidly.

Cheese is generally considered as a substance insoluble in water, and yet a great part of it is in actual solution in milk. A solution of it in water may be obtained, if cheese precipitated by an acid and well expressed, be digested with carbonate of barytes or carbonate of lime. The carbonate is decomposed with effervescence, and the cheese quitting the acid is dissolved. The solution is yellowish, and resembles a solution of gum. Evaporated to dryness it leaves a yellow mass, which easily redissolves in water. The solution boiled in an open vessel, becomes covered with a white pellicle, precisely as milk does, and acquires the smell of boiled milk. The membrane is almost insoluble in water, and appears to be a product by the action of air on the dissolved cheese.

With the mineral acids cheese produces the same combinations as albumen and fibrin, although the neutral combinations are less soluble than those of fibrin. A great excess of acetic acid is required in order to dissolve the cheese, and the neutral combination with this acid appears to be insoluble. Cheese is easily dissolved in alkalies. Its solution in acetic acid, as well as in ammonia, becomes covered with a small quantity of cream every time that the cheese has not been well separated from the butter. Alcohol converts cheese into an adipocirous and fœtid substance.

Butter and sugar of milk are so well known, that no additional information can result from my experiments on them.

ARTICLE VI.

Memoir on the Determination of the Specific Heat of the different Gases. By MM. F. Delaroche, M.D. and J. E. Berard.

(*Continued from* p. 377.)

SECTION VI.

Determination of the Specific Heat of the Gases, that of Water being taken for Unity.

We have already observed, that the principle upon which our first set of experiments to determine the specific heat of the gases, was founded, furnished us with two methods of ascertaining the ratio between their specific heats and that of water, supposing it unity. The second process, described in § III., for the basis of which we are indebted to Count Rumford, furnished us with a third method of arriving at the same conclusions. We will describe in succession the result of our experiments, according to each of these three methods.

§ I.—*First Method.*

In determining the specific heat of the gases, we set out from this principle, that this specific heat was proportional to the maximum of the elevation of temperature in the calorimeter by a current of hot gas. To compare these specific heats with those of water, it was only necessary to compare the effect produced upon the calorimeter by one of the gases, and by a current of hot water moving so slowly that its effects should not be much more considerable than those of the gas. This we did in the following manner ;—

To get a constant and regular current of water, we made use of a syphon of glass, C, (Plate X. fig. 9,) one of the branches of which passed through one of the mouths of a flagon with two mouths almost filled with water. On the other mouth was placed a globular vessel turned upside down, and full of water. The neck of this vessel was of such a length as just to reach the surface of the water in the flagon Hence it is evident, that whenever the water ran out through the syphon so as to sink its surface below the mouth of the globular vessel, the water which filled that vessel would fall down and supply its place ; so that the height of the water in the flagon would remain always nearly the same, and of course the water would flow through the syphon pretty equably. The bore of the syphon was capillary, in order to make the current of water very slow.

This current of water was heated in the same manner as the current of the different gases in the preceding experiments: but when a current of hot water circulates slowly in a horizontal tube, it is impossible to determine its temperature exactly by means of a thermometer placed in the centre of the tube, on account of the unequal temperature of the different strata of fluid which constitute the current. We ascertained, by experiment, that a thermometer thus placed indicated a temperature much lower than the true temperature of the current. To get rid of this uncertainty, we disposed the apparatus in the manner represented in fig. 9.

The current of water which flows from the syphon, C, is received into the tube, D D, which passes through a larger tube, E E, kept continually filled with steam, by means of the small boiler, R. The tube, D D, is soldered to another tube, F F, about 0·3 inch in diameter.* In this tube, placed, as will be seen, almost vertical, are disposed three thermometers, Nos. 1, 2, 3, in such a manner that the small spherical bulb of each is in the centre of the tube, and at the distance of 1·083 inch from each other. We shall see below how these thermometers served to determine the temperature of the current of water at its entrance into the calorimeter. The tube, F F, is cemented to the horizontal tube of glass, G G, which is cut at its end, G, very near the junction, so that the bulb of the thermometer No. 1, which must necessarily be in the vertical tube, that it may acquire the temperature of the current of water, is, notwithstanding, no farther distant from the calorimeter than the first thermometer is from the third. The tube, G G, is shut by a stopper, H, that no water may escape, and it is ground at its extremity, G, so that by pressing it, by means of two screws, against a piece of leather placed on the calorimeter, it is sufficiently tight to prevent any water from escaping in that direction. The water comes out of the calorimeter by the tube, O O O, which is capillary at its extremity, P, from which it falls, drop by drop, into a graduated tube, which serves to measure the rapidity of the current. The calorimeter was placed, as in the preceding experiments, in a separate room, that the temperature of the ambient air might be less variable.

The same method was always followed to determine the maximum effect produced by the current of hot water. The temperature of the calorimeter was in the first place raised to a temperature a little below the maximum. The current of hot water was then made to pass through it, and the temperature marked every ten minutes. When its effects were scarcely any longer greater, the thermometer stood as in the following table,

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* This tube is represented separately in the plate of a larger size.

Temperature of the air surrounding the calorimeter.	Temperature of the calorimeter.	Thermometers.		
		No. 1.	No. 2.	No. 3.
45·990°	83·025°	140·954°	151·286°	145·742°

The temperature of the calorimeter was then artificially raised about 1·1°, and the current of water made to circulate as before. The temperature of the calorimeter gradually sunk; and when it had nearly reached its minimum, the following observations were made:—

Temperature of the surrounding air.	Temperature of the calorimeter.	Thermometers.		
		No. 1.	No. 2.	No. 3.
45·966°	83·503°	140·756°	151·340°	145·598°

In these experiments the current of water was such that 583·01 grains troy of water passed in ten minutes.

If we examine with attention the state of the thermometers Nos. 1, 2, 3, we will perceive, that for distances so small as those in our apparatus, the following proposition may be considered as true. When a regular current of hot water passes through a vertical tube, if thermometers be placed in its centre at equal distances from each other, the difference between two neighbouring thermometers will be proportional to the excess of the temperature of the water above that of the surrounding air.

We cannot therefore commit an error of much importance, if we suppose that the current of hot water, in going from thermometer No. 1 to the calorimeter, lost 4·86 of its heat, and entered into the calorimeter at the temperature of 135·59°.

We ascertained, by experiment, that it issued out at the same temperature as that of the calorimeter.

It follows from this experiment that a current of water amounting to 583·01 grains in ten minutes, by losing 52·330° of its heat, kept the calorimeter higher than the surrounding air by 35·283°.

We see by the first table that 2196·4 cubic inches of air, or 723·71 grains, passing through the calorimeter every ten minutes, and losing 130·347° of heat, kept its temperature elevated above the ambient air 28·321°. If we bring by calculation the results of the experiment on the current of water, to what they would have been under the same circumstances as in the current of air, we find that the maximum of temperature at which it would have maintained the calorimeter would have been 115·241° above the surrounding air. The specific heats being proportional to the effects of the currents, it follows that if the specific heat of water be 1, that of air is 0·2460. A second experiment gave us 0·2536. The mean of the two is 0·2498.

§ II.—*Second Method.*

The second method of knowing the ratio between the specific heats of water and air, consists in determining by calculation the quantity of heat given out in a certain time by the calorimeter, when the current of hot air has rendered its temperature stationary. We have shown that, when it reaches this point, it abandons as much heat to the ambient air as it receives from the hot gas. The bases of this calculation are as follows:—

Our calorimeter contained 8586·8 grains of distilled water. The brass of which it was composed and the worm weighed 5065·6 grains. Now as the specific heat of this metal is 0·112,[*] this quantity of brass corresponds with 566·80 grains of distilled water. There was besides 1652·5 grains of solder, the specific heat of which being a mean between that of tin and lead[†] may be reckoned at 0·038. Hence the solder is equivalent to 62·70 grains of water. Therefore the whole calorimeter, with its contents, contained as much heat as 9217 grains of distilled water.

Let us recapitulate here the result of our experiment upon atmospherical air under the pressure of 29·922 inches of mercury, and at the temperature of 32°. A current of air of 2196·4 cubic inches in ten minutes, by cooling 130·347°, lost a quantity of heat sufficient to keep the calorimeter 28·321° above the temperature of the surrounding air. Hence it furnished it in a given time with as much heat as it lost during that time: but it results from an experiment of which we shall give an account immediately, that if the quantity of heat which it lost in ten minutes under these circumstances had not been supplied, its temperature would have sunk 2·8793°; or, which comes to the same thing, the quantity of heat abandoned by the current was sufficient to elevate the temperature of the calorimeter 2·8793°.

The air which passed through the calorimeter in ten minutes weighed 723·71 grains. This quantity, in suffering a diminution of temperature amounting to 130·347°, lost as much heat as was sufficient to raise 9217 grains of water 2·8793°; or, which comes to the same thing, to heat 203·60 grains of water, 130·347°. Hence the specific heat of air is to that of water as 203·60 to 723·71 ; or as 0·2813 to 1 ; a ratio which does not differ much from that which we obtained by means of a direct current of water.

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[*] Crawford. Thomson's Chemistry, ii. 227, French transl.

[†] The specific heats of lead and tin are taken from the Memoirs of Lavoisier, i. 130.

It now remains to prove, that if the heat lost by the calorimeter in ten minutes had not been supplied by the hot gas, its temperature would have sunk 2·8793°.

We began by determining, by means of the following experiments, how much the temperature of the calorimeter would have sunk, after having reached its maximum, in a given time, if the current of hot gas had been suddenly stopped. For this purpose, the temperature of the calorimeter was raised, by means of a lamp, to about 104°. Then leaving every thing in the same situation as when the current of hot gas was circulating, even the tube, F G, (fig. 5) which served to heat the gas, being kept full of steam, but not passing any current of gas through the apparatus, we left it to itself, and marked the rate of its cooling. By this method we obtained, in two successive experiments, the two following series :—

First Series.

Time.	Temperature of the air.	Temperature of the calorimeter.
0′	41·333°	86·454°
5	41·333	84·412
10	41·333	82·436
15	41·310	80·620
25	41·310	78·872
30	41·286	77·216
35	41·194	75·652
40	41·103	74·165
50	40·917	71·398
55	40·917	70·104
60	40·887	68·913
65	40·795	67·752
70	40·795	66·643

Second Series.

Time.	Temperature of the air.	Temperature of the calorimeter.
0′	41·540°	84·007°
5	41·472	82·076
10	41·466	80·227
15	41·309	78·467
20	41·265	76·829
25	41·265	75·249
30	41·265	73·778
35	41·286	72·408
40	41·286	71·056
45	41·274	69·795
50	41·265	68·610
55	41·265	67·474

Now we see by our first table that the current of atmospherical air there indicated raised the temperature of the calorimeter 28·321° above the influence of the steam tube employed to heat the gas, or, which comes to the same thing, above the influence of the ambient air, supposing it increased by 4·5° (the effect of the steam tube). Let us make choice in our first series of observations on the rate of cooling, a term, whose excess above the temperature of the surrounding air, supposed likewise increased by 4·5° (to render the circumstances similar) is nearly the same. We find 74·165°. Neglecting the terms immediately following, let us take the one observed 20 minutes after, namely, 68·913. If we subtract it from the other term, we get 5·252°, a quantity which expresses the cooling of the calorimeter in 20 minutes. We find by a proportion that if the excess of the temperature of the calorimeter above that of the surrounding air, augmented always by 4·5°, had been in the case which we have chosen exactly 28·321, the cooling in 20 minutes would have been 5·197°, and of course this excess would have been found at the end of 20 minutes equal to 23·124°.

This diminution of temperature, given by experiment, is not the same with that which the calorimeter would have experienced in the same time, if it had preserved the rate of cooling which it had at first. It would have been more considerable; but we may determine it by means of the preceding data, and by making use of the following formula:—

$$S = A \text{ log. hyp. } \frac{A}{B} \; *$$

in which S signifies the quantity wanted; A, the excess of the temperature of the calorimeter above that of the surrounding air in the first instant of the experiment; and B, that excess at the end of 20 minutes.

If we apply this formula to the numbers stated above, we have A = 28·321°, and B = 23·124°, which gives us S = 5·7411°. We find S = 5·7760°, if we make use of the second series of observations of the cooling of the calorimeter. The mean of these two values gives us S = 5·7586°, and of course $\frac{S}{2}$ = 2·8793° = the quantity of heat lost in ten minutes, supposing it to preserve always the initial rate of cooling. This is the number which we employed to calculate the relation between the specific heat of air and water.

§ III.—*Third Method.*

The experiments which we made, according to the process contrived by Count Rumford, gives us a direct and more simple method of determining the specific heat of air compared with that of water. We see by the table of these experiments that, under a pressure of 29·922 inches of mercury, and at the temperature of 32°, 5077·5 cubic inches of atmospherical air, or 1672·9 grains, by undergoing a diminution of 154·192° of temperature, raised the temperature of the calorimeter 7°. But in these experiments our calorimeter contained 8957·5 grains of distilled water. If to this number we add 628·56 grains, which, as we have seen above, represents the quantity of metal in the calorimeter, we have 9586·06 grains of water, which we may consider as containing as much heat as the calorimeter.

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* See Note 3d, at the end of this paper, for an explanation of this formula.

1672·9 grains of air, by losing 154·192° of heat, increase the temperature of 9586·06 grains of water 7°: but we find by the rule of proportion that 9586·06 grains, to be heated 7°, require as much heat as 451·18 grains to be heated 154·192°. Hence the specific heats of air and of water are to each other in the ratio of 451·18 to 1672·9, or of 0·2697 to 1.

By the first method we found the specific heat of air 0·2498, by the second 0·2813. We see that these three determinations do not differ much from each other. By taking a mean of them, we get 0·2669 for the specific heat of air, that of water being considered as unity. This being determined, it is easy, by means of the first table, to refer the specific heats of all the gases to that of water. Under a pressure of 29·922 inches of mercury, these specific heats are as follows:

Water	1·0000
Air	0·2669
Hydrogen gas	3·2936
Carbonic acid	0·2210
Oxygen	0·2361
Azote	0·2754
Oxide of azote	0·2369
Olefiant gas	0·4207
Carbonic oxide	0·2884
Aqueous vapour	0·8470

Section VII.

General Considerations.

We shall terminate this paper by pointing out the general result of the facts which it contains, and by offering some observations on the consequences which may be drawn from them.

§ I.—*Specific Heats of the Gases compared with each other.*

The specific heat of the gases is not the same for every one, whether we attend to their volumes or their weights. The differences, it is true, are not very great in the first point of view. Hence, when very accurate experiments are not made, it is easy to be misled (as some philosophers have been) into the notion that the gases do not differ from each other in this respect. We have not observed any relation between the specific gravity and the specific heats of the gases. The following table is a sufficient proof of this:—

	Specific Heats of		
	Equal volumes.	**Equal weights.**	**Specific gravity.**
Air	1·0000	1·0000	1·0000
Hydrogen	0·9033	12·3401	0·0732
Carbonic acid	1·2583	0·8280	1·5196
Oxygen	0·9765	0·8848	1·1036
Azote	1·0000	1·0318	0·9691
Oxide of azote	1·3503	0·8878	1·5209
Olefiant gas	1·5530	1·5763	0·9885
Carbonic oxide	1·0340	1·0805	0·9569

The difference between the specific heats of the gases, when the same weights of both are taken, is more considerable, as is obvious from the table.

§ II.—*Influence of the Density on the Specific Heat of the same Gas.*

The specific heat of atmospherical air in the ratio of its volume, increases with its density, but at a slower rate: of course, considered in the ratio of the mass, it diminishes as the density increases.

This is the direct result of our experiments, which give us the specific gravity of air, subjected to the pressure of 41·654 inches of mercury, 1·2396; while that of the same volume of air, subjected to the pressure of 29·634 inches of mercury, was 1·0000. Here the increase of specific heat is in the ratio of 1 to 1·2396, while that of the pressure is in the ratio of 1 to 1·3583.

It results, likewise, from this experiment, that the ratio of the pressures being the same as before, the specific heats of the same mass of air, subjected to these different pressures, are in the ratio of 1 to 0·9126.

Every person knows that when the air is compressed heat is disengaged. This phenomenon has been long explained by the change supposed to take place in its specific heat: but this explanation was founded upon mere supposition, without any direct proof. The experiments which we have given seem to us sufficient to remove all doubts on the subject.

Dr. Irvine, and several philosophers since his time, have endeavoured to determine the real zero, or the number of degrees that a body must be cooled in order to be deprived of the whole of its heat. He set out from this principle, that the quantity of heat disengaged, or absorbed, during the passage of a body from one state to another, is entirely owing to the change of capacity which takes place in the body: and he conceived that he could, knowing this change of capacity, and the quantity of heat absorbed, or given out, during this change, determine the whole quantity of heat which the body contained. Thus, supposing the specific heat of ice 0·9, and that of water 1·0, and admitting that water in freezing gives out 135° of heat, it has been concluded that the real zero is 1350° below the freezing point.

Different motives, which it would be too tedious to state here, but some of which we shall notice below, have induced philosophers to entertain doubts respecting the justice of the principles upon which this determination depends. We are disposed to consider these doubts as well founded. At the same time, we think it worth while to exhibit a calculation of the same kind derived from our experiments.

It is well known that the theory of sound, founded on the knowledge of the elasticity of the air, gives for the rate of its propagation a velocity less considerable than we obtain from experiment. M. de Laplace conceives that this difference between theory and observation is owing to having neglected the increase of elasticity occasioned by the heat evolved in consequence of the sudden compression of air during the transmission of sound: and M. Poisson has proved that, to make the results of theory and observation agree, we must suppose that a volume of air, when it is suddenly compressed by a force capable of diminishing its bulk $\frac{1}{116}$th part, gives out a quantity of heat sufficient to raise its temperature 1° centigrade (1·8° Fahr.). This supposition is so probable, that we may consider that measure of the quantity of heat disengaged by the compression of air as more exact than what we could obtain by a direct experiment. We may therefore employ this datum to determine the quantity of heat which air would have abandoned in passing from the pressure of 29·154 inches of mercury to that of 41·654 inches, to both which pressures it was subjected in our experiments. We find that it would have been sufficient to have raised the temperature of the same quantity of air 54·9°.* Now let us denote by $b\ x$ the absolute quantity of heat contained in the mass of air compressed, b being its specific heat, and x the excess of its temperature above the real zero. Let us denote by $a\ x$ the quantity of heat contained in the air not compressed, a being its specific heat. The excess of one of these quantities above the other will be $(a - b)\ x$; and since, according to the theory of Dr. Irvine, this excess is equal to the quantity of heat disengaged during the change of state of the body, we may represent it in the present case by 654·9°, since b is the specific heat of the compressed air. Therefore $(a - b)\ x = 54{\cdot}96$. This gives us $x = \frac{54\cdot96}{a\,b}$. Substituting for a and b their values given us by experiment, namely, 1 and 0·9126, we get $x = 573\cdot1^\circ$. If we subtract from this quantity 19°, the number of degrees above 32° that the experiments were made, we get 553·9° for the number of degrees of heat contained in ice at the temperature of 32°. This result differs very materially from what we obtained by comparing the specific heats of water and ice; but it does not differ much from 675°, which we should obtain, if, with Dr. Irvine, we considered the specific heat of ice to be 0·8; but we again repeat that we attach very little importance to this kind of reasoning.

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* Supposing the disengagement of heat proportional to the absolute diminution of volume.

The difficulty of this sort of experiments has prevented us from determining if the change in the capacity for heat by pressure be the same in all gases. This is exceedingly probable, as the increase of density of each from pressure is the same. Hence the opinion ought to be admitted till new experiments demonstrate to the contrary. We have no direct proofs of it, however. The very curious experiments of M. de Saissy,* if they are exact, may even induce us to entertain doubts on the subject.

§ III.—*Specific Heat of the Gases compared with that of Water, and with different Solids and Liquids.*

We cannot make experiments to determine the specific heats of the gases without remarking that, when equal volumes are considered, they are very small, compared to the specific heats of liquid and solid bodies. The most careless experiment is sufficient to prove the justice of this assertion, which a more exact examination fully confirms. Thus, if we compare the specific heat of an equal volume of olefiant gas (which under the same volume has the greatest specific heat) and of water, we find that the first is only $\frac{1}{2000}$th part of the second.

If we take the same weight of each, the specific heat of the gases approaches much more nearly to that of the solid bodies, as may be seen from the result of our experiments, which we here lay before our readers:—

	Specific heat.
Water	1·0000
Air	0·2669
Hydrogen	3·2936
Carbonic acid	0·2210
Oxygen	0·2361

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* M. de Saissy, a philosopher of Lyons, has made experiments, from which it follows, that when gases are subjected to a strong and sudden pressure those only which contain oxygen give out light, and oxygen itself gives out the most.

	Specific heat.
Azote	0·2754
Oxide of azote	0·2369
Olefiant gas	0·4207
Carbonic oxide	0·2884
Vapour of water	0·8470

From this table it appears that, if we except hydrogen, which has the greatest specific heat of all known bodies, all the gases that we have examined have a smaller specific heat than water, and a greater specific heat than any of the metals.

The results which we have obtained by comparing the specific heat of the gases with that of water, enable us to decide whether, as some have thought, it would be attended with a saving of fuel, to employ the action of dilated air, instead of steam, in steam-engines. We consider the question here under a point of view entirely theoretic, abstracting both the difficulty of constructing such machines, and the loss of power which could not be entirely avoided. Setting out from the specific heats of water and air contained in the preceding table, we have found that with the same quantity of heat employed in the one case to convert water of 32° into steam, but without raising its temperature higher than 212°, and in the other to bring the temperature of atmospherical air from 32° to 212°, the effects produced in the first case would be to those produced in the second case as 1 to 1·285: but the advantage in favour of air would be much greater if the temperature were raised still higher.* It is obvious that, from the knowledge which we already possess of the quantity of heat given out by steam when it is condensed, and from the data furnished by our experiments on the specific heats of the gases, it was very possible to arrive at the solution of this question; but the calculations being somewhat complicated, and requiring, in order to be presented with clearness, details which might appear foreign to the subject proposed by the Institute, we will not give them here.

§ IV.—*Specific Heat of the Vapour of Water compared with that of Water itself.*

It would have been very interesting to have determined with accuracy the specific heats of different vapours, and to have compared them with the fluids which they form by condensation. The prodigious quantity of heat disengaged during this change of state, leads one to believe, if the opinion of Irvine on the subject be adopted, that the capacity of vapours for heat is much greater than that of the corresponding liquids. The solution of this question would probably be the most certain means of throwing light on this important subject. Unfortunately, our experiments are very few, owing to the great difficulty of this kind of research. We have confined ourselves solely to the vapour of water, nor have we determined its specific heat with the utmost precision. Our experiments lead to the conclusion, as will be seen in the preceding table, that the specific heat of vapour is 0·847, that of water being 1·000. It would be necessary to conclude from it, that the specific heat of water, so far from increasing when it passes into vapour, undergoes a diminution. This would be sufficient to overturn the whole doctrine of Irvine. This conclusion would be similar to that which we have drawn in the following paragraph, with infinitely more certainty, relative to the heat disengaged, or absorbed, during the combinations of bodies. We acknowledge, however, that though we consider our determination of the specific heat of vapour as not far from the truth, we have not such an entire confidence in it, as to draw from it a conclusion of such importance without hesitation.

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* Thus, if instead of applying the same quantity of heat to raise a mass of air from 32° to 212°, it was employed to raise the temperature of ¼ of it from 32° to 312°, the effect produced in this case would be 3·043, or thrice as great as would be produced by employing the same quantity of heat to convert water into steam.

§ V.—*Specific Heat of the Gases compared with that of the Compounds into which they enter.*

When during the combination of two bodies there take place a condensation and a disengagement of heat, the compound has a smaller specific heat than that which we should obtain by a calculation founded on the specific heats of the two constituents. When, on the other hand, there is a production of cold, the compound has a greater specific heat than is indicated by calculation.* This, at least, is the result of almost all the experiments hitherto made on the subject. Two facts only are known to constitute an exception to this general law: and we may very well suppose, that both are owing to some error in the observation. They were both observed by Lavoisier and de Laplace; the one on the combination of lime with water, the other in the solution of nitre in water.

Dr. Irvine, of Glasgow, the first discoverer of this law, and who was unacquainted with the two facts above stated, deduced from it a very ingenious hypothesis, by means of which he explained the disengagement of heat which takes place in combinations. He supposes that this disengagement is entirely due to a diminution in the capacity of the new compound for heat. This hypothesis, which is merely an expansion of that by means of which he explained the emission and absorption of heat during the change of state of bodies, is liable to very strong objections, which have prevented the greater number of philosophers from adopting it; but which were not quite decisive, and therefore insufficient to destroy the hypothesis entirely. It has still several partizans. Dr. Crawford adopted it, and it was some years ago explained in detail by Dr. Irvine, jun.

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* This calculation is very simple. Let 1 be the mass of the compound, a one of its constituents; $1 - a$, the other; c, the specific heat of the first constituent; d, that of the second. The specific heat of the compound is $ac + (1 - a)d$.

All the facts relative to the specific heat of bodies being proper to throw light on this question, we shall examine in this point of view some of those which result from our experiments.

1. Oxide of azote (composed, according to Davy, of 0·638 azote and 0·367 oxygen).

Specific heat of, by calculation ....0·2404
By our experiments ....0·2369

2. Carbonic acid (composed of carbonic oxide 0·634, and of oxygen 0·366, *Mem. d'Arcueil*, ii. 253).

Specific heat of, by calculation .....0·2692
By our experiments ....0·2210

3. Red oxide of lead (composed of lead 0·9, oxygen 0·1, Berzelius, *Ann. de Chim.* lxxviii. 14).

Its specific heat by calculation (that of lead being 0·0282).......... 0·0490
By the experiments of Lavoisier and Laplace .... 0·0622

4. Red oxide of mercury (composed of 0·85 mercury, and 0·15 oxygen, Chenevix).

Its specific heat by calculation (that of mercury being 0·029) ...... 0·0600
By the experiments of Lavoisier and Laplace .... 0·001

5. Water (composed of 0·87 oxygen, 0·13 hydrogen).

Its specific heat by calculation ......0·6335
By experiment ....1·0000

If we now examine how far this difference between experiment and calculation corresponds with the law of Dr. Irvine, we shall find that, for the three compounds, 1, 2, 4, they enter into the general law; for though it has not been directly observed that there is a disengagement of heat during the formation of these bodies, the condensation, which the elements experience in forming these combinations, proves that such a disengagement must have really taken place. This is not the case with the third example, the red oxide of lead. We ought equally to believe that heat has been disengaged during its formation; and yet its specific heat is somewhat greater than the mean. It is true, that as the difference is but small, and as it is derived from experiments liable to error, we cannot draw from it a positive objection against the law: but the fifth case exhibits an example which appears to us perfectly decisive. The specific heat of water by calculation ought to be only 0·6333, while in reality it is 1·0000. The only part of this determination against which any doubts can be raised, is the specific heat of oxygen and hydrogen gases: but we conceive that the methods which we employed were such that we could not fall into an error with respect to these gases nearly so great as would be necessary to make the difference between experiment and calculation disappear; still less that the difference should be on the other side, as would be necessary to make our results agree with the law of Irvine. Supposing all the causes of error to have acted the same way, and supposing them to exceed the limits which it is reasonable to assign them, we could not raise the specific heat of water by calculation higher than 0·800. It is necessary, therefore, unless we deceive ourselves, to abandon the hypothesis which ascribes the evolution of heat in cases of combination to a diminution of the specific heat in the bodies combined, and admit, with Black, Lavoisier, and Laplace, and many other philosophers, the existence of caloric in a state of combination in bodies. The knowledge of the specific heat of oxygen alone would be sufficient to induce us to adopt this opinion: for it is so small that it is almost impossible for us to account for the great quantity of heat disengaged during the combustion of the greatest number of bodies, unless we suppose that this heat previously existed in a state of combination. Accordingly, when the opposite hypothesis was adopted, philosophers were obliged to suppose the specific heat of this gas fifteen times greater than it is in reality.

We must not suppose, however, that there exists no relation between the specific heat of compounds and that of their constituents. Too many facts prove this relation to make it possible to deny it. Water, in this respect, constitutes the greatest deviation which has been observed; yet it does not exceed ⅓d of the specific heat of this fluid. In general, we may say that the constituents of a body communicate to it their specific heat. This is very observable in the combinations of hydrogen, which has the highest specific heat of all known bodies. The compounds which it forms have a much greater specific heat than other bodies. Hence the great specific heat of water, of olefiant gas, and of animal and vegetable substances.

NOTE 1st.

We employed, to determine the quantity of heat communicated to the calorimeter by the vapour tube which served to heat the gases the same process as that which gave us the maximum temperature to which the current of hot gas brought the calorimeter We made vapour pass through the tube, D E, (fig. 5) without any current of gas, and observed the rate at which the calorimeter heated. We stopped the experiment when the increase of heat in the calorimeter was become exceedingly slow. Then we raised the heat of the calorimeter artificially about 2 degrees, thus raising it above the maximum point, and continued the experiment till it had almost ceased to sink. We took the mean between these two terms as the effect produced by the vapour tube. This experiment was made twice with great care. In the first experiment the ascending series conducted us to 51·8° (the air being at 46·778°), and the descending series to 53·042° (the air being at 46·634°). If we take the mean between these two numbers, we get 5·58° for the maximum effect produced by the vapour tube on the calorimeter. The second experiment gave us exactly the same number.

It may be asked, if the steam tube will communicate as much heat to the calorimeter when the steam ot gas is passing as when it is absent. It is easy to see that it will not, since the temperature of the tube, D E, remaining the same, the calorimeter is hotter in the one case than in the other: and it is a well-known principle, that when one body communicates heat to another, the quantity communicated is the more the greater the difference of temperature between the two bodies.

It is very difficult in the present case to determine how much the heat communicated by the steam tube will be diminished by the increase of temperature of the calorimeter. Fortunately, the quantity is so small that it is of little importance. We may suppose, without risking any error of importance, that the quantity of heat yielded is as the difference between the temperature of the calorimeter and of the tube, D E. This difference being 144° in the experiments which we have related, and producing an elevation of temperature amounting to 5·58° in the calorimeter, this elevation ought to be 4·5°, when the difference between the temperature of the steam tube, D E, and of the calorimeter is only 129·6°, as was the case in the experiments which we made on the different gases.

NOTE 2d.

We determined in the following manner at what degree the current of hot air maintained the calorimeter stationary.

In consequence of data furnished by a preliminary experiment, we raised the temperature of the calorimeter to 76·359°. The thermometer, indicating the temperature of the air of the room, stood at 44·88°.

The calorimeter was then wiped with care, that no unknown cause might accelerate its cooling, and the current of hot gas was made to pass through it. The experiment was continued till the thermometer of the calorimeter, observed every ten minutes, almost ceased to rise. The observations were as follows ;—

Temperature of the air.	Temperature of the calorimeter.
44·830°	76·369°
44·830	76·460
44·841	76·548
44·910	76·626
44·929	76·696
44·953	76·787
44·969	76·847
44·969	76·879
44·969	76·896

From this table we see that the current of hot air was capable of heating the calorimeter only about 0·017° the last ten minutes. The calorimeter was then raised to the temperature of 78·073°. The current of hot air was continued, and the thermometer observed every ten minutes. The results were as follows:—

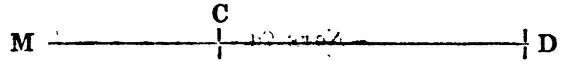
Temperature of the air.	Temperature of the calorimeter.
45·199°	78·073°
45·176	78·035
45·153	77·940
45·153	77·857
45·153	77·806
45·153	77·790
45·176	77·770

The calorimeter during the last ten minutes sunk only 0·02°. We obtain the maximum effect by taking the mean of the results furnished by these two sets of observations.

The result is, that the current of hot atmospherical air rendered the calorimeter stationary at the temperature of 77·383°, that of the ambient air being 45·172°.

Note 3d.

We may compare the cooling of a hot body placed in air whose temperature is constant to the motion of a body, M, which goes in a direct line towards a fixed point, D, with a velocity always pro-



portional to the space, M D, which remains to be passed over. The fixed point, D, is the temperature of the surrounding air, and the space, M D, is the variable excess of the temperature of the hot body above that of the surrounding air. Hence we will apply to the motion of such a body the reasoning on which is founded the formula cited in the paper.

This formula is an expression for the space, S, which the body setting out from C, the distance of which from D is known, will pass over in the time, T; supposing it to preserve during the whole motion the initial velocity, V, which it had in C. Nothing would be simpler than the determination of this space, if V were known; for we should have $S = V T$: but it is not given immediately by experiment. We must therefore endeavour to determine it by joining, to the data which we have already, that which we obtain by the knowledge of the space which the body setting out from the point, C, passes over with a variable velocity, v, which at first is equal to V. This space is given us by experiment. We shall express it by $A - B$: A being the known distance, C D, and B the space that remains to be passed over at the end of the time, T. For the greater convenience, let us suppose for a moment that it is the space $A - B$ that we have to determine, B being unknown, and V being known. Its expression will be $d\ (A - x)$, or $- d x = v\, d t$, x being the general expression of the space which remains to be passed over each instant, v that of the velocity at the same time, and t that of the time elapsed. But since at each instant the velocity is proportional to the space that remains to be passed over, we may make $v = k x$, k being a constant quantity which may be determined by making $k A = V$. We shall have then $- d x = k x\, d t$, and $- \frac{dx}{x} = k dt$. Integrating the sides of this equation, we obtain $-$ log. hyp. $x + C = k t$. To determine the constant quantity, C, make $x = A$, which suppose the time, t, to be nothing. We get $-$ log. $A + C = o$, and $C =$ log. hyp. A. Substituting this value of C in the preceding equation, we shall have $-$ log. $x +$ log. A, or log. $\frac{A}{x} = k t$, $\frac{A}{x} = e^{kt}$ (e being the number whose hyperbolic logarithm is unity; $x = \frac{A}{e^{kt}}$, and $A - x = A - \frac{A}{e^{kt}}$. Applying this equation to the time given, T, x will become B, and we shall have $B = \frac{A}{e^{kt}}$, and $A - B = A - \frac{A}{e^{kt}}$.

Let us suppose, now, that B and $A - B$ are given by experiment, and that V, or, which comes to the same thing, k, is unknown. To discover its value, it is sufficient to make use of the equation, which on the preceding hypothesis gave the value of $A - B$, supposing B known; or still more simply to employ the equation log. $\frac{A}{x} = k t$, to which we came by putting for x and t their values B and T. We obtain from it $k = \frac{\text{log. } \frac{A}{B}}{T}$, $V = A \frac{\text{log. } \frac{A}{B}}{T}$, and $S = A$ log. $\frac{A}{B}$. We have seen above that S was the space passed over by the body with a uniform velocity, V, during the time, T.

To apply this formula to the cooling of a hot body, we must denote by A the excess of its temperature above that of the air at the beginning of the experiment; and by B, the same excess at the end of the time, T.

Article VII.

Essay on the Cause of Chemical Proportions, and on some Circumstances relating to them: together with a short and easy Method of Expressing them. By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

I. *On the Relation between Berthollet's Theory of Affinities and the Laws of Chemical Proportions.*

Some chemists have affirmed that the existence of chemical proportions is contrary to the principles of the theory of affinities with which the illustrious Berthollet has enriched chemistry. On that account they have refused to adopt it. But if, on the one hand, the knowledge of chemical proportions, which we at present possess, does not accord with all the applications made by Berthollet, and by other chemists, of his theory; on the other hand, it is incontestible, that these principles have never been refuted, but are more and more confirmed, the more they are examined. Chemists, before Berthollet, were misled by considering the weakest of the two chemical forces, or affinities, opposing each other as null. Berthollet pointed out that error, and showed the effect which the chemical *mass* produces.

Berthollet himself, far from denying the possibility of chemical proportions, has contributed a good deal to prove their existence, although the numbers resulting from his analytical experiments be not always very accurate. He has proved that when the elements cease to oppose each other, in consequence of their chemical mass, their combinations always take place in definite and invariable proportions. The doubts entertained by some chemists of the truth of Berthollet's principles originate certainly from the conduct of some of his zealous supporters, who have extended his doctrine to cases to which it does not apply, and have maintained the existence of indefinite combinations even when the action of the chemical mass cannot interfere. This opinion no doubt occasioned the fine experiments of Proust, whose object was to show that, when the metallic oxides absorb more oxygen, they pass at once from one degree of oxidation to another, without passing through the intermediate steps; and that what had been considered as an intermediate step was merely a mixture of a perfect oxide with an imperfect one.

The effects of the chemical mass are produced when, for example, three bodies, A, B, and C, exist together in the same solution; that is to say, in mutual contact; and when both A and B have an affinity for C, and endeavour to form the new combinations, A C and B C, which still remain in solution, and of course preserve their contact with the surplus of A and

B, which exists in the solution. If it has the strongest affinities, it combines with a greater proportion of C than B does: but though the attractions between A and C be greater than those between B and C, A is not able to draw to itself the whole of C, to the exclusion of B. In such a case, the portion of C combined with A to that combined with B will be determined by the strength of the affinity, and by the proportion of each present in the solution. Any person may satisfy himself of the reality of this division of C, by pouring concentrated muriatic acid into a solution of sulphate of copper. The solution, which was formerly blue, acquires a green colour by the addition of the muriatic acid (for muriate of copper is green); and this colour becomes stronger the more muriatic acid is added. The cause of this phenomenon is, that the affinity of the muriatic acid, though the weaker, still continues to act, and acts with more intensity the greater the quantity of that acid present. If, for example, 100 parts of sulphuric acid, and 100 parts of muriatic acid, divide 100 parts of oxide of copper between them, so that the sulphuric acid takes 80 parts and the muriatic 20 parts, it is clear that the force with which 100 of sulphuric acid remain in combination with 80 of oxide is equal to the force with which 100 muriatic acid remain in combination with 20 of oxide; that is to say, that the two opposite forces are in equilibrio. If any one of these combinations should separate from the solution—if the sulphate of copper, for example, should begin to crystallize; the phenomena would change. A new force, crystallization, would be added. This force does not act merely by abstracting a part of the chemical mass, but likewise as a positive force, capable of counterbalancing and of being counterbalanced.

It is obvious that in all this there is nothing inconsistent with the laws of chemical proportions. But it will be said that the 100 parts of sulphuric acid combined with the 80 parts of oxide of copper, and the 100 of muriatic acid with the 20, are not combined according to the laws of chemical proportions. It is obvious that the surplus of each of these acids is employed in counterbalancing the surplus of its antagonist, and cannot be considered as combined with the oxide of copper. Of consequence, the part of each acid really combined with the oxide is neutralized according to the laws of chemical proportions. I conceive that this single example is sufficient to show that the principles of Berthollet's theory are not inconsistent with the laws of chemical proportions.

II. *On the Cause of Chemical Proportions.*

The fact that bodies combine in definite proportions when other forces do not oppose their re-union, added to the observation that when two bodies, A and B, combine in different proportions, the additional portions of the one are always multiples by whole numbers, 1, 2, 3, 4, &c. lead us to conclude the existence of a cause in consequence of which all other combinations become impossible. Now *what is that cause?* It is obvious that the answer to this question must constitute the principal basis of chemical theory.

When we reflect on this cause it appears at first evident that it must be of a mechanical nature; and what presents itself as the most probable idea, and most conformable to our experience, is, that bodies are composed of atoms, or of molecules, which combine 1 with 1, 1 with 2, or 3, 4, &c.; and the laws of chemical proportions seem to result from this with such clearness and evidence, that it seems very singular that an idea so simple and so probable has not only not been adopted, but not even proposed before our own days. As far as I know, the English philosopher, Mr. John Dalton, guided by the experiments of Bergman, Richter, Wenzel, Berthollet, Proust, and others, was the first person who endeavoured to establish that hypothesis. Sir H. Davy has lately assured us that Mr. Higgins, in a book published in 1789, established the same hypothesis. I have not seen the work of Mr. Higgins, and can only notice the circumstance on the authority of Davy.*

Notwithstanding the great clearness and simplicity which characterize this hypothesis, it is connected with great difficulties, which make their appearance when we apply it to a number of chemical phenomena. These difficulties naturally excite doubts as to the truth of the hypothesis. Among the numerous experiments which I have made in order to discover the chemical proportions in which bodies unite, I have met with cases when, notwithstanding the completest agreement with the laws which I conceived myself to have discovered, the composition of a body could not be explained according to the hypothesis which we are considering. I shall state some of these cases, without, however, considering them as absolute proofs against the hypothesis, but rather as difficulties which we must endeavour to surmount, in order to obtain a clear and well established theory of chemical proportions.

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* The work of Higgins on *Phlogiston* is certainly possessed of much merit, and anticipated some of the most striking subsequent discoveries. But, when he wrote, metallic oxides were so little known, and so few exact analyses existed, that it was not possible to be acquainted with the grand fact that oxygen, &c. always unite in determinate proportions which are multiples of the minimum proportion. The atomic theory was taught by Bergman, Cullen, Black, &c., just as far as it was by Higgins. The latter, indeed, states some striking facts respecting the gases, and anticipated Gay-Lussac's theory of volumes; but Mr. Dalton first generalized the doctrine, and thought of determining the weight of the atoms of bodies. He showed me his table of symbols, and the weights of the atoms of six or eight bodies, in 1804; and I believe the same year explained the subject in London in a course of lectures delivered in the Royal Institution. The subject could scarcely have been broached sooner. But about the same time several other persons had been struck with the numbers in my table of metallic oxides published in my *Chemistry*; and the doctrine would have certainly been started by others if Dalton had missed it.—T.

I shall begin with a short explanation of the corpuscular theory, such as I conceive it. I shall employ the word *atoms* to signify the corpuscles, or smallest parts of which bodies are composed. When I say the *smallest parts*, I mean that they cannot be divided into other parts still smaller. I do not enter into any discussion whether matter be infinitely divisible or not, but take it for granted that an atom is mechanically indivisible; and of course that a fraction of an atom cannot exist. I suppose likewise that atoms are all spherical, and that they have all the same size. (This last circumstance is not necessarily attached to the idea of atoms, but it is absolutely necessary if regular figures are to result from their union, and if they unite in definite proportions, even in the most complicated combinations). It appears likewise necessary that when an atom of the body, A, combines with one or more atoms of the body B, to form a new compound atom, the atom of A touches each of the atoms of B. Hence a compound atom is formed by the juxtaposition of several elementary atoms; just as an *aggregate* is formed by the juxtaposition of different homogeneous atoms. But the difference consists in this, that in the first case an electric discharge takes place of the specific polarity of the heterogeneous atoms, which cannot take place between homogeneous atoms. (See my conjectures on this subject in Nicholson's Journal for March 1813, p. 154.)

A compound atom, for very obvious reasons, cannot be considered as spherical; but as it is composed of atoms mechanically indivisible, or which cannot be separated by mechanical means, the compound atom is justly as completely mechanically indivisible as the elementary atom. It is likewise evident that an atom composed of A + 3 B ought to be greater, and to have a different figure from an atom composed of A + B. The former ought to have the form of a triangular and equilateral pyramid, while the latter must have a linear form.

We may divide the atoms into two classes: 1. Elementary atoms: 2. Compound atoms. The compound atoms are of three different species; namely, 1. Atoms formed of two elementary substances united. We shall call them *compound atoms of the first order*. 2. Atoms composed of more than two elementary substances: and as these are only found in organic bodies, or bodies obtained by the destruction of such organic matter, we shall call them *organic atoms*. 3. Atoms formed by the union of two or more compound atoms, as, for example, the *salts*. We shall call them *compound atoms of the second order*.

The greatest number of spherical atoms of the same diameter, capable of touching a single atom of the same diameter, is 12. Hence it follows that A + 12 B contains the greatest number of atoms which a *compound atom of the first order* can contain. If, on the other hand, we pay attention to the electric polarity of the atoms, an atom of A cannot combine with more than 9 atoms of B, if the atom A + 9 B preserve any part of the electric polarity originally belonging to A; for example, *oxymuriatic acid,* which is a compound of 1 atom of muriatic radicle and 8 atoms of oxygen, still preserves a part of the original polarity of the radicle, by means of which it re-acts; while the *supersulphuret of arsenic,* of which I shall give an account in the sequel, and which is composed of 1 atom of arsenic and 12 atoms of sulphur, has no other electrochemical re-action than that of sulphur.

It is contrary to sound logic to represent a single compound atom of the first order as composed of 2 or more atoms of A combined with 2 or more atoms of B; as, for example, 2 A + 2 B, 2 A + 3 B, 7 A + 7 B, &c.: for in such a case there is no obstacle, either mechanical or chemical, to prevent such an atom from being divided, by means purely mechanical, into 2 or more atoms of more simple composition. Besides, such a composition would almost totally destroy chemical proportions. Hence it follows, that in stating the result of an analysis conformably to the views of the corpuscular theory, we must always consider one of the constituents as unity, that is to say, as a single atom. What I have stated here appears to me to be necessary consequences, or reflections inseparable from the theory of atoms, not one of which can be rejected without committing what is called *contradictio in adjecto.*

I shall now give an account of the difficulties to which I conceive the corpuscular theory is liable.

1. The first of these difficulties is, the circumstance that there are combustible bodies, *iron,* for example, which unite only with two doses of oxygen, the second of which is only 1½ times greater than the first. This difficulty, however, is only apparent; for I have already, in my former memoirs on this subject, shown that, in all probability, it is owing to our being still unacquainted with all the degrees of oxidation of which the body in question is capable. The multiple 1½ implies the existence of an inferior degree of oxidation to that which we consider as the minimum. I hope in this essay to prove the truth of this opinion in a still more satisfactory manner.

2. I think I have proved that when two oxides combine they always unite in such proportions that each contains either an equal quantity of oxygen, or the one contains a quantity which is a multiple by a whole number of the oxygen in the other.

This law, though in itself conformable to the corpuscular theory, admits, on the one side, of combinations inconsistent with that theory; and, on the other hand, it excludes combinations perfectly conformable with that theory. I shall explain this by an example. Let O be oxygen, A and B two combustible bodies. The law which we are considering admits of a combination of $A + 3\,O$ with $1\frac{1}{2}\,B\,O$, because $1\frac{1}{2} \times 2 = 3$: and we shall see immediately that such combinations exist, though, according to the corpuscular theory, they appear absurd. On the other hand, the law does not admit the combination of $A + 3\,O$ with $B + 2\,O$, though such a combination be conformable to the theory of atoms. The black oxide of copper is composed, according to our present knowledge, of 1 atom of metal and 2 atoms of oxygen, and sulphuric acid of 1 atom of sulphur and 3 atoms of oxygen. We know that there is a subsulphate of copper in which the acid and the oxide contain each equal quantities of oxygen. Of course, this subsulphate must contain for every atom of sulphuric acid an atom and a half of oxide of copper. It is true that we may object to this, that there is some appearance that sulphuric acid is composed of 6 atoms of oxygen to 1 of sulphur. But I shall have occasion to discuss this opinion when I come to speak particularly of sulphur. Arsenic acid, from new experiments of which I shall give an account in the sequel, is composed of 1 atom of arsenic and 6 atoms of oxygen. The yellow oxide of lead is composed of 1 atom of metal and 2 atoms of oxygen. The arseniate of lead is composed in such a manner that the acid contains three times as much oxygen as the oxide, that is to say, of an atom of acid and an atom of oxide. The subarseniate of lead is composed in such a manner that the acid contains twice as much oxygen as the oxide; that is to say, of an atom of acid and an atom and a half of oxide.

If we suppose that the yellow oxide of lead contains but one atom of oxygen, this subsalt ceases to be an objection to the atomic theory; but in that case we meet with an equally formidable objection in the composition of the red oxide of lead.

I have endeavoured to prove that two different oxides of the same radicle sometimes combine in such a manner that each contains an equal quantity of oxygen, or that the one contains two, three, &c. times as much as the other. Among these combinations there are some which do not agree with the hypothesis of atoms: for example, the red oxide of iron contains 3 volumes of oxygen, and the black oxide 2 volumes. Gay-Lussac has lately found that the oxide of iron formed at a high temperature by the action of the vapour of water is composed of 100 iron + 37·8 oxygen. But this combination of the two oxides is composed in such a manner that the red oxide in the compound contains exactly twice as much oxygen as the black: that is to say, that it is composed of 1⅓ atom of the first and 1 atom of the second. One method of refuting this objection would be to consider the black oxide of iron as containing 4 atoms of oxygen, the red as containing 6, and the intermediate oxide as containing 5: but in this case the analysis of Gay-Lussac is incorrect. He should have obtained 36·8 of oxygen instead of 37·8. But we are not at present acquainted with any example of a body containing 5 atoms of oxygen; and I shall prove in the sequel that the oxide in question cannot be considered as a particular oxide, since it possesses all the characters of a compound of the red and black oxides of iron.

3. We have seen that an elementary atom cannot combine with more than 12 elementary atoms. Inorganic nature has not yet presented us with any body which is inconsistent with this supposition: but among organic bodies such examples are very frequent. It is in the study of the composition of organic bodies that our knowledge of the laws of chemical proportions, and of the electrochemical theory, will one day reach that degree of perfection which the human mind is capable of giving it. I shall give the composition of oxalic acid as an example of the constitution of an organic atom. I analysed this acid by decomposing it, by distilling oxalate of lead mixed with a quantity of brown oxide of lead, and making the gaseous products pass through muriate of lime, and then through limewater. I repeated this analysis with so little variation, that I consider my results as a close approximation to the truth. In neither of these analyses did I obtain as much water as amounted to a quantity of hydrogen equivalent to 1 per cent. of the acid: but we cannot conceive an atom of oxygen to be united with a fraction of an atom of hydrogen. We must therefore consider the small quantity of hydrogen which we obtain as an entire atom. If we admit water to be a compound of 2 atoms of hydrogen and 1 atom of oxygen, and carbonic acid of 1 atom of carbon and 2 atoms of oxygen, it follows from my analysis, that the atom of oxalic acid is composed of 1 atom of hydrogen, 27 atoms of carbon, and 18 atoms of oxygen; that is to say, that it consists of an atom of hydrogen combined with 45 other atoms.

If, on the other hand, we chuse to consider the organic atoms as consisting of an atom of compound radicle combined with 1 or more atoms of oxygen, and of course oxalic acid as composed of an atom of radicle and 3 atoms of oxygen: the radicle in that case will be a compound of 1 atom hydrogen + 27 atoms carbon, and will remain equally inapplicable to the hypothesis of atoms. It follows, likewise, that an atom of oxalic acid is eleven times greater than an atom of sulphuric acid, and fifteen times greater than an atom of potash: yet in the superoxalate of potash discovered by Dr. Wollaston an atom of potash should be combined with 1¼ atom of oxalic acid.

I own that I do not know how these observations, and several others which it would be superfluous to mention here, can be reconciled with the hypothesis of atoms: but it would be rash to conclude that we shall not be able hereafter to explain these apparent anomalies in a satisfactory manner. Till that time comes, the hypothesis of atoms can neither be adopted nor considered as true.

I have already, in preceding memoirs, made mention of another method of viewing chemical proportions—a method founded on a fact discovered by Gay-Lussac; namely, that bodies when in the state of gases unite either in equal volumes, or 1 volume of one combines with 2, 3, &c. volumes of the other. This fact has been already verified by several distinguished chemists. From what we know respecting definite proportions, it follows, that it would hold with all bodies in the temperature and pressure at which they would assume the gaseous form. Hence there is no other difference between the theory of atoms and that of volumes, than that the one represents bodies in a solid form, the other in a gaseous form. It is clear, that what in the one theory is called an *atom*, is in the other theory a *volume*. In the present state of our knowledge the theory of volumes has the advantage of being founded upon a well constituted fact, while the other has only a supposition for its foundation. In the theory of volumes we can figure to ourselves a demi-volume, while in the theory of atoms a *demi-atom* is an absurdity. On the other hand, the theory of volumes has a disadvantage from which the atomic theory is free; namely, the existence of compound bodies, especially of an organic nature, which we cannot suppose ever to have existed in the form of gas.

I ought to observe, that we have here, as well as in the theory of atoms, *elementary volumes* and *compound volumes of the first and second order*. It follows from the laws of chemical proportions, that two compound volumes, containing a common constituent, ought to combine in such a manner that they contain either equal volumes of this common constituent, or that the one contains two, three, &c. times the number of volumes of the other. It is almost demonstrated that an elementary volume never combines with 1¼ volume of another elementary substance: but at present we are obliged to admit that this sometimes happens with compound volumes.

In the theory of volumes we cannot suppose the combination of 2 volumes with 3, &c.: for on such a supposition there can be no reason assigned why 4 volumes should not combine with 5, 7 with 9, 999 with 1000, &c.: so that in such a case no reason could be assigned for the existence of chemical proportions. Here, as well as in the theory of atoms, it is absolutely necessary that in each compound one of the constituents should be considered as a single volume.

It is evident that if the weight of the volumes of the elementary bodies be known, and expressed in numbers, we have nothing more to do in every case of analysis but to count the relative number of volumes of the constituent parts, whatever the form of their aggregation may be: but in order to obtain the relative weights of the elementary volumes expressed in numbers, that is to say, to obtain their specific gravity in the form of gas, we must have a general measure with which we may compare them. We may chuse among the elementary bodies one, the weight of a volume of which must be denoted by unity; just as water has been chosen for unity in determining the specific gravity of liquids and solids.

There are only two elementary bodies possessed of the requisite qualities to serve as our unit. These are *oxygen* and *hydrogen*. But hydrogen has disadvantages from which oxygen is free. The weight of a volume of hydrogen is so small, that if we employ it as our unit, the number representing a volume of some of the metals becomes inconveniently great. Besides, hydrogen enters much less frequently into compounds than oxygen; and of course the number 100, when applied to hydrogen, does not nearly so much facilitate calculation as when it is applied to oxygen. Add that oxygen constitutes among elementary bodies a particular class, and, as it were, the centre round which chemistry turns. It exists in the greater number of unorganic bodies, and without exception in all the products of organic nature. I think, then, that it is at once most convenient, and most agreeable to the scientific views of chemistry, to take oxygen as our unit. I shall represent its volume by the number 100.

The question which we have now to resolve is this, *What is the specific gravity of all other elementary bodies in the form of gas, compared with that of oxygen?* This question is not easily answered: for at present there are no other bodies except oxygen and hydrogen which we are capable of weighing in the state of gas. All other bodies are converted into gas at such high temperatures that it is not in our power to ascertain their weight. We must therefore endeavour to discover the weight of their volumes by other means. Our results will be, doubtless, very uncertain; but not altogether unsuccessful; as I hope to be able to show in the sequel.

In the first place, it appears reasonable to suppose that bodies ought to combine most generally in equal volumes: but in examining the greatest number of the combinations of elementary bodies, we find that those which are distinguished by a strong affinity between their constituent parts, and by the force of their chemical affinity for other bodies, contain evidently more than one volume of one of their elements. This is the case with water, carbonic acid, nitrous gas, &c., and, with very few exceptions, it is always the electro-negative element the volume of which is multiplied. On the other hand, in bodies composed distinctly of equal volumes, such as the nitric suboxide (*azote*), and carbonic suboxide (*carbonic oxide*), we find all the negative properties which characterize the suboxides. This leads me to suppose that all the suboxides are composed of equal volumes of their elements. It follows from these observations that the most part of the salifiable oxides and acids ought to be composed of more than one volume of oxygen for each volume of radicle.

Experiment seems to prove that if a combustible radicle combine in preference with 2 or 3 volumes of oxygen, it combines likewise in preference with 2 or 3 volumes of sulphur. If a salifiable oxide be composed of 1 volume of radicle and 2 or 3 volumes of oxygen; and if we neutralize this oxide by any acid whatever, it is to be supposed that the neutral combination which results ought to contain for 1 volume of the radicle of the oxide as many volumes of the radicle of the acid as the oxide contains volumes of oxygen; and, consequently, that the number of times which the acid contains the oxygen of the oxide will be the number of volumes of oxygen combined with 1 volume of the radicle of the acid: for example, we consider sulphuric acid as composed of 1 volume of radicle and 3 volumes of oxygen; because it is very probable that the quantity of sulphur and of oxygen capable of combining at an elevated temperature with a given portion of lead constitute equal volumes. But if we want to know by another method how many volumes of oxygen exist in sulphuric acid, we have only to examine the composition of some sulphate; for example, sulphate of iron (*sulphas ferrosus*). The black oxide of iron contains 1 volume of metal and 2 volumes of oxygen. It follows, from what has been said, that the black oxide of iron ought to be neutralized by a quantity of acid containing 2 volumes of sulphur for every volume of iron: so that the number of volumes of the sulphur of the acid and of the oxygen of the base shall be equal. But the acid contains three times as much oxygen as the base; consequently, it is composed of 3 volumes of oxygen and 1 volume of sulphur. If, instead of the sulphate of iron, we were to make choice of the persulphate of iron (*sulphas ferricus*), it is evident that in such a case the iron is combined with 3 volumes of sulphur, so that the result is just the same.

This observation would be sufficient to determine the volume of a substance whose oxide possesses the characters of an acid, or of an electro-negative body, were it not for the numerous exceptions which exist to the rule, some very remarkable instances of which I shall have occasion to notice in the sequel. Hence it is always necessary, in order to discover those exceptions, and to verify the weight of the volume sought to compare the result of the preceding calculation with the known degrees of oxidation of the substance whose volume is wanted. If, for example, we find that an acid or electro-negative oxide is neutralized by a quantity of base or electro-positive oxide which contains $\frac{1}{3}$ of the oxygen in the acid, this acid will appear to contain 3 volumes of oxygen. But if among the oxides of the radicle of this acid we find one which contains half the oxygen of the acid, it is clear that the saline combination in question is an exception, and that the acid must contain 6 instead of 3 volumes of oxygen. I refer, for a farther explanation of this, to what I shall say in the sequel concerning *arsenic* and *chromium*.

The preceding observations explain why, when a salifiable base has combined with more oxygen, it requires always an additional volume of acid for every volume of oxygen which it has absorbed. It is for the same reason that oxygen appears to determine exclusively the composition of bodies; though there can be no doubt that every element contributes equally to that composition.

While treating in the sequel of each particular substance, I shall explain the way in which I determine the weight of a volume of it, and likewise state the experiments on which the calculation is founded. As none of our experiments, except from accident, can be perfectly correct, and as a small error in the result often increases in the calculation, it is not possible that my determinations can be perfectly exact: but I hope to approach within very near limits, at least, of the truth. The difference in the analytical results will point out to us the limits of error, and show us degrees beyond which our determinations cannot be incorrect. I shall give an account of these minima and maxima indicated by experiment, as well as of the experiments themselves, which point them out. We have, for example, every reason to believe that a volume of sulphur weighs 201: but some experiments raise it as high as 210, while others sink it as low as 200. As we cannot determine at present which of these numbers is most exact, it is good to know within what limits our knowledge is uncertain.

As far as I know, the English chemists Dalton, Davy, and Young, are the only persons who have yet attempted to make these determinations; and they have proceeded in a manner somewhat different. Mr. Dalton, to whom the honour of the first attempt is due, has endeavoured to determine the relative weights both of simple and compound atoms. (*New System of Chemical Philosophy.*) Davy, though he has not adopted the atomic theory of Dalton, has embraced the doctrine of definite proportions; and what Dalton calls an *atom,* he calls a *proportion.* (*Elements of Chemical Philosophy.*) Dr. Young, in his *Introduction to Medical Literature,* has made similar determinations; but what Davy calls *proportion,* Young calls *combining weight.* But none of these philosophers have attempted to give any great degree of exactness to their determinations. They have frequently even omitted stating the experiments from which these determinations are derived. The method which they have adopted of giving round numbers, though it facilitates the recollection and calculation, is scarcely consistent with the object of scientific researches, and ought to be rejected; for even supposing that perfect exactness could never be obtained, it is nevertheless the object towards which all our efforts should be directed.

(To be continued.)

Article VIII.

Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Magnetical Observations.

Latitude 51° 32′ 40″ North. Longitude West in Time 6″ $\frac{83}{100}$.

1813.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.		Variation.			Hour.		Variation.			Hour.	Variation.
Oct. 18	8h	45′	24°	16′	36″	1h	55′	24°	21′	10″		Discontinued, from the shortness of the days.
Ditto 19	8	50	24	15	32	2	00	24	21	11		
Ditto 20	8	45	24	15	40	1	55	24	21	50		
Ditto 21	—	—	—	—	—	1	45	24	25	00		
Ditto 22	8	45	24	14	44	—	—	—	—	—		
Ditto 23	8	45	24	15	17	2	00	24	23	36		
Ditto 24	8	45	24	16	05	1	55	24	22	32		
Ditto 25	8	55	24	15	25	1	55	24	21	06		
Ditto 26	—	—	—	—	—	2	00	24	25	44		
Ditto 28	8	30	24	15	24	—	—	—	—	—		
Ditto 31	8	35	24	22	25	2	10	24	23	50		

Mean of Observations in Oct.	Morning	at	8h	45′	Variation	24°	15′	41″	West.
	Noon	at	1	59	Ditto	24	22	53	West.
	Evening	at	—	—	Ditto	—	—	—	Not obs.
Ditto in Sept.	Morning	at	8	53	Ditto	24	15	46	West.
	Noon	at	2	02	Ditto	24	22	32	West.
	Evening	at	6	03	Ditto	24	16	04	West.

Mean of Observations in Aug.	Morning at 8h 44′.....	Variation 24° 15′ 58″	West.
	Noon at 2 02.....	Ditto 24 23 32	
	Evening at 7 06.....	Ditto 24 16 08	
Ditto in July.	Morning at 8 37.....	Ditto 24 14 32	West.
	Noon at 1 50.....	Ditto 24 23 04	
	Evening at 7 08.....	Ditto 24 13 56	
Ditto in June.	Morning at 8 30.....	Ditto 24 12 35	West.
	Noon at 1 33.....	Ditto 24 22 17	
	Evening at 7 04.....	Ditto 24 16 04	
Ditto in May.	Morning at 8 22.....	Ditto 24 12 02	West.
	Noon at 1 37.....	Ditto 24 20 54	
	Evening at 6 14.....	Ditto 24 13 47	
Ditto in April	Morning at 8 31.....	Ditto 24 09 18	West.
	Noon at 0 59.....	Ditto 24 21 12	
	Evening at 5 46.....	Ditto 24 15 25	

Magnetical Observations continued.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Nov. 2	8h 50′	24° 17′ 24″	—h —′	—° —′ —″		Discontinued from the shortness of the days.
Ditto 4	8 25	24 18 11	— —	— — —		
Ditto 5	8 25	24 18 37	— —	— — —		
Ditto 6	8 25	24 17 03	— —	— — —		
Ditto 7	8 35	24 19 00	1 50	24 23 51		
Ditto 8	8 22	24 16 03	— —	— — —		
Ditto 9	8 25	24 15 56	— —	— — —		
Ditto 10	8 25	24 16 51	— —	— — —		
Ditto 11	8 25	24 43 55	— —	— — —		
Ditto 12	8 35	24 17 25	— —	— — —		
Ditto 13	8 50	24 19 20	1 50	24 21 17		
Ditto 14	8 35	24 17 50	1 55	24 21 03		
Ditto 15	8 50	24 16 37	1 55	24 22 00		
Ditto 17	8 40	24 17 07	1 47	24 21 50		

Oct. 20.—The needle at noon vibrated at times six minutes: the next day the wind blew from the S. E. with rain.

Nov. 11.—The variation extraordinary this morning, in the evening there was a violent storm from the S.W. which lasted 36 hours, and the gusts were remarkable for their strength and frequency.

Nov. 15.—At noon the needle was steady, until it began to rain; it then vibrated five minutes and ten seconds.

Nov. 17.—At the commencement of the snow storm the needle vibrated eight minutes and thirty seconds.

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* Rain fallen { Between noon of the 1st Oct. / Between noon of the 1st Nov. } 3·661 inches.

Evaporation during the same period, 1·020 inches.

Article IX.

Account of a luminous Meteor seen at Sunderland. By Mr. Renney.

(To Dr. Thomson.)

SIR, *Bishopwearmouth, Nov.* 12, 1818.

Permit me, through the medium of your Journal, to lay before your readers the following particulars of what I consider a very uncommon phenomenon; in the hope that you, or some other scientific Gentleman, will have the goodness to afford an explanation through the same channel.

On Wednesday evening, the 10th of this month, about 40 minutes after six, the moon having just risen, but was not visible, owing to a very dense cloud (whose altitude was 7°), the most opaque I ever recollect to have seen. From behind this cloud there appeared a stream of light, which extended across the hemisphere, and so dense that γ in Pegasus, and α in Aquila, were obscured by it; the edges of which at intervals diverged into lines, diminishing its breadth: but, that I may afford a more perfect idea, I shall give its extent by the stars that appeared in or near the northern and southern edges:—

On the Northern Edge.

Taurus.	Aries.	Pegasus.	Dolphin.	Aquila.
α λ	ν	α	ε	γ

On the Southern Edge.

Taurus.	Cetus.	Pisces.	Pegasus.	Aquila.
μ ο	λ	ε	β	β

It was obscured by a dark cloud in Taurus Poniatowski, and extended visible through Serpentarius to the horizon.

I immediately waited upon Thomas Jones, Esq. a Gentleman of considerable scientific acquirements, who possesses a very excellent astronomical and philosophical apparatus (to whom I am under many obligations for their use); and although he was at home, and disengaged, I found that before he could come to the door this immense volume of light had lost much of its lustre, and was so reduced in substance that γ in Pegasus now became visible near the centre. Mr. Jones observed, that he "had seen many uncommon phenomena of nature, but none like the present." Its duration, I believe, did not exceed six minutes; for, at the time I first saw it, I think it was then in its greatest splendour, but in about three minutes it entirely disappeared.

In the north the Aurora Borealis was faintly playing, from which to the south the heavens (except the cloud before alluded to, and a cloudy horizon) were clear, and had the appearance of frosty weather. The wind at the time was easterly, and much rain had fallen during the day.

I am of opinion that this phenomenon could not be caused by the above-mentioned cloud concentrating the rays of the rising moon; for if it had, it could not have caused it either to extend across the whole hemisphere, nor to have had that density which this possessed: and at the same time I may remark, that the moon was not the centre; but, on referring to a celestial globe, will be found to have been to the north of it; consequently, if the moon had caused this luminous appearance, it would have been in a different direction; besides, the stream of light was not in a right line, but rather bearded to the north.

I am, with much respect, Sir,
Your most obedient humble servant,
Robert Renney.

Article X.

Analyses of Books.

A short Account of Experiments and Instruments depending on the Relation of Air to Heat and Moisture, 8vo. By John Leslie, F.R.S.E. Professor of Mathematics in the University of Edinburgh.

Mr. Leslie has already gained considerable reputation by his curious experiments on the effect of surface on the rate of cooling or heating, which he published about nine years ago. In the present little tract he gives us his opinions respecting the nature and effects of heat in general; and makes us acquainted with some important facts which he has ascertained respecting evaporation, the dryness and moisture of air, and the mode of producing cold by evaporation. He describes a variety of instruments which he has contrived for this kind of investigation. The book is written with considerable perspicuity; and, as far as I can judge, the style is greatly preferable to that of Mr. Leslie's other literary productions. No great attention has been paid to accuracy of arrangement; and Mr. Leslie introduces without ceremony (as has always been his habit) any topic which strikes his fancy, whether it bears upon the subject in hand or not. Perhaps the subjects treated of in this work are of so unconnected a nature, that they were not easily susceptible of a better arrangement than our author has given them: but there was a very obvious method of destroying the want of connection which always looks ill in a philosophical work. The book might have been divided into as many chapters as there were subjects discussed. This would have rendered the whole much more perspicuous and entertaining; and, I am persuaded, would have made the book much more generally read than it will be. In its present state it is best adapted for those who are already pretty well acquainted with the doctrine of heat. Had it been divided into chapters, it would have been easily understood, so perspicuously is it written, by every person in the least curious about such subjects. In my analysis of the book I shall take the liberty (for the sake of my readers) of supplying Mr. Leslie's omission, and dividing it into its various heads.

I. *Sketch of the Facts respecting Heat.*—Mr. Leslie, with the greater number of philosophers of the present day, thinks that the phenomena of heat are best explained by considering it to be a fluid of a very peculiar nature, either the same as that which produces light, or a modification of it. He gives us a short sketch of the facts known respecting the conducting power of different bodies, and their different capacities for heat. He adopts 135° for the latent heat of water. This is the number which Mr. Cavendish informs us he obtained by his own experiments. From the well-known accuracy of Mr. Cavendish, there is every reason to believe that his number is entitled to be preferred to 140°, the number which Dr. Black deduced from his experiments. At the same time, it were to be wished that Mr. Leslie had informed us of the reason which induced him to adopt 135° in preference of 140°. Did he repeat the experiment himself, and obtain 135°? Or did he rely solely upon the authority of Mr. Cavendish?

Mr. Leslie accounts for the heat evolved during combustion by the change of capacity which the substances concerned in combustion undergo. Thus when charcoal is burnt, the oxygen of the air is changed into carbonic acid; and the superiority of the capacity of oxygen gas above that of carbonic acid is the reason of the enormous quantity of heat evolved during the combustion. This is the mode of explanation which was first given by Dr. Black, and afterwards adopted by Dr. Irvine, and Lavoisier, and Laplace. I endeavoured about ten years ago to show that it was quite inadequate to account for the phenomena; and I still consider my arguments as perfectly conclusive. The new experiments of Berard and Delaroche on the specific heat of the gases put the inadequacy of the explanation adopted by Mr. Leslie, and the philosophers just named, in a very striking point of view. The specific heat of oxygen gas is 0·2361, while that of carbonic acid is 0·2210: so that the oxygen has a specific heat $\frac{23}{1000}$th parts higher than the carbonic acid. Let us suppose a pound of charcoal consumed. From Lavoisier's experiments it appears that the heat evolved is sufficient to melt 96½ lbs. of ice. Now this (supposing Mr. Cavendish's estimate correct) is equal to 13027½ degrees of heat. The oxygen consumed amounts to 2·8 lbs. nearly: so that each pound of oxygen, when changed into carbonic acid, must have given off 3428 degrees. Here a change in the specific heat amounting only to $\frac{23}{1000}$, or not quite $\frac{1}{43}$d of the whole, occasioned the escape of 3428 degrees. Such a conclusion can only be adopted if we suppose the absolute quantity of heat in the oxygen gas to amount to 147404 degrees. This supposition exceeds the estimate of Dr. Crawford nearly 100 times; and it is more than ten times greater than that adopted by Dalton. No person can believe that oxygen gas contains so much heat. Of course the supposition that the heat evolved during combustion is owing to a change of capacity merely cannot be defended. If heat be a fluid, it must enter into chemical combination with certain bodies, and the decomposition of these combinations must be the cause of the heat evolved during combustion.

Mr. Leslie's notions of the capacity of the different gases for heat, as stated in this treatise, are much more accurate than those of his predecessors, though they do not agree with the results obtained by Delaroche and Berard. Thus he makes the specific heat of hydrogen gas about 10 times greater than common air. Delaroche and Berard make it rather more than 12 times greater.

II. *Evaporation.*—The facts stated by Mr. Leslie respecting evaporation are curious, and some of them are new. What struck me as the most novel and important of his discoveries on this subject is, that the rate of evaporation, like that of the escape of heat from bodies, depends upon the nature of the surface: or that the escape of water by evaporation, and the escape of heat from the surfaces of bodies, depend upon the same law. Water evaporates fastest from those bodies that allow heat to escape fastest, and slowest from those that allow heat to escape slowest; and it follows exactly the same rate as the escape of the heat. Hence from the surface of glass, charcoal, or paper, water evaporates much faster than it does from metals.

It would appear from this, that it is the radiant heat chiefly that occasions evaporation. Probably the heat that escapes by the conducting power of the neighbouring bodies is acted upon by the affinity of these bodies, and on that account cannot combine with the moisture, and convert it into steam.

Another important fact with which Mr. Leslie makes us acquainted is the rate at which the capacity of air to retain vapour increases with its temperature. According to him, this capacity is doubled for every 27 degrees of heat added to air. Air at the temperature of 32° is capable of holding $\frac{1}{160}$th of its weight of vapour, at 59° it becomes capable of holding $\frac{1}{80}$th part, at 86° $\frac{1}{40}$th part, at 113° $\frac{1}{20}$th part, and at 140° $\frac{1}{10}$th part. These estimates do not agree with those previously given by Mr. Dalton, from whose table it appears that the quantity of vapour which air can contain increases at a greater rate than Mr. Leslie makes it do.

Mr. Leslie adopts the old opinion respecting the state of vapour in the atmosphere. He conceives that it is held in solution by the air precisely as salt is by water. He does not inform us of the reasons that induce him to adopt this opinion: but the result of his own experiments, if he had attended to it, would have shown him that the supposed analogy between the solvent power of water and air does not in reality exist. The solvent power of air, he says, is doubled by every additional 27 degrees of heat. But can any case be produced in which the solvent power of water increases in any similar proportion? I believe not.

Mr. Leslie in this part of his book makes us acquainted with a number of instruments which he has invented for different purposes. The great principle upon which these instruments act is in most cases the same, though the information which they convey be different. This is probably the reason why they have been described all together. It would have been better if each instrument had been described in a separate chapter, or at least a separate section. These instruments are,

1. The *hygrometer.*—This instrument is merely the differential thermometer, having one ball covered with bibulous paper and silk, and the other made of coloured glass. The paper is moistened. The evaporation of this water produces cold, and this cold increases with the rapidity of the evaporation. Now this rapidity depends upon the comparative dryness of the surrounding air. Hence the lower the liquid falls in the tube of the hygrometer, or the greater cold produced, the drier is the air. Mr. Leslie gives us a curious collection of facts obtained by his observations with this instrument.

2. The *Pyroscope.*—This is an instrument for measuring the intensity of a fire. It is merely the differential thermometer with one of its balls covered with silver leaf, while the other is naked. The fire heats the naked ball, but not the silvered ball. Hence the liquid in the tube rises or falls according to the intensity of the fire, and of course marks that intensity.

3. The *Photometer.*—This is the differential thermometer, having one of its balls of colourless glass, the other of black glass. The light produces no effect upon the transparent ball,

but it heats the black ball according to its intensity; and this heat, by depressing the liquid in the tube, marks the intensity of the light. Mr. Leslie has given a curious and valuable collection of facts established by means of this instrument; some of them published long ago in Nicholson's Journal; others new. I should have been sorry had these curious facts been omitted, which, in strict propriety, they ought to have been, as they have no connection whatever with the subjects which the book professes to treat of.

4. The *Atmometer.*—This instrument has been already described in the *Annals of Philosophy*, vol. i. p. 467. It is an imperfect instrument; because it gives us no immediate information by inspecting it, as all the other instruments do. We must observe it twice in succession, and know the time between the two observations, before we can draw any conclusions from it.

5. *Ivory Hygrometer.*—This instrument has also been described in the *Annals of Philosophy*, vol. i. p. 468. The construction of it is highly ingenious, and does credit to Mr. Leslie's inventive powers: but it is from its very nature imperfect; and is always liable to alter by time. These imperfections were not unknown to Mr. Leslie, who points them out with great candour.

III. *Drying Power of different Bodies.*—Mr. Leslie took different earths and various other bodies, dried them well before the fire, and then put them into well stopped phials. By putting a little of each into a large glass bottle along with a hygrometer, he ascertained the degree of dryness which it produced in the air. These results are curious, and unexpected. They are as follows:—

Alumina made the hygrometer stand at	84°
Carbonate of magnesia	75
Carbonate of lime	70
Silica	40
Carbonate of barytes	32
Carbonate of strontian	23
Pipeclay	85
Greenstone	80
Sea sand with shells	70

The degree of dryness produced by the different soils he found proportional to the fertility of the soil. Hence he conjectures that the fertility depends upon this property. There can be no doubt that the fertility of soils is very much connected with the power which they possess of retaining the proper degree of moisture.

Connected with these curious results are the following:—

100 grains of ivory attract from the air 7 grains of humidity
boxwood 14
down 16
wool 18
beech 28

There is little difference between the effect produced upon the dryness of air by the pure earths and their carbonates; but the effect is greatly diminished by heating the earthy bodies red-hot.

IV. *Cold produced by Rarefaction, assisted by the absorbing Power of Sulphuric Acid.*—Mr. Leslie's method of freezing water by placing it under the receiver of an air-pump in a small vessel, while the greatest part of the bottom of the receiver is occupied by a shallow vessel of sulphuric acid, and exhausting the receiver, is known, I presume, to most of my readers. By drawing out the air, the rate of evaporation is accelerated. The sulphuric acid absorbs the vapour as fast as it is formed. Thus the evaporation is continued without intermission. The cold produced is sufficient to freeze the water in a very short time: indeed, it is so great that Mr. Leslie has even frozen mercury by means of it. Mr. Leslie describes the different experiments which he has made on this curious subject at considerable length, explains the theory of the congelation, points out the proper methods of proceeding, and the various important purposes to which the process may be applied. This part of the book is very interesting, and highly deserving the attention of all classes of readers. Indeed, the many valuable practical inferences which the author draws in different parts of his book, and the many useful purposes to which he conceives his instruments may be applied, give this little treatise a claim upon the attention of the nation in general, and entitle the author to the thanks, not only of men of science, but likewise of our manufacturers and agriculturists.

The Philosophical Transactions for 1813, part ii. are published: they shall be noticed in a subsequent Number of the *Annals of Philosophy*.

The Memoirs of the Imperial Academy of Sciences of St. Petersburgh for 1809, 1810, and 1811, have been received in London: they shall be likewise noticed in a future Number of the *Annals of Philosophy*.

ARTICLE XI.

Proceedings of Philosophical Societies.

ROYAL SOCIETY.

The Royal Society resumed its meetings on Thursday, the 4th of November. A description of a sliding scale of chemical equivalents, contrived by Dr. Wollaston, was read. This contrivance is distinguished by all that sagacity which characterizes Dr. Wollaston, and cannot but be highly useful to the practical chemist. It is not easy to render the nature of this contrivance intelligible without an engraving, which I do not consider myself at liberty to give before the publication of the paper in the Philosophical Transactions; but the following observations will perhaps suggest some idea of it to those who are interested in practical chemistry.

It was first observed by Richter that when two neutral salts are made to decompose each other, the neutrality of neither is disturbed. Thus if you dissolve 100 grains of sulphate of potash in water, and pour into the solution muriate of barytes in sufficient quantity to decompose the whole of the sulphate, two new neutral salts will be formed, namely, sulphate of barytes and muriate of potash. If into the muriate of potash thus formed a sufficient quantity of nitrate of silver be dropped to decompose it, two new neutral salts will be formed, namely, nitrate of potash and muriate of silver. Thus the same weight of potash has been united in succession with sulphuric, muriatic, and nitric acids. The weight of barytes that neutralized the muriatic acid neutralized likewise the weight of sulphuric acid combined with the potash which had neutralized the sulphuric acid. These observations of Richter were still farther generalized by Berthollet; but it is to Dalton that we owe the full generalization of the facts, and the explanation of them. He supposes that bodies unite atom to atom, and showed how the weights of the atoms of bodies might be determined. This subject, having been already explained at considerable length in the *Annals of Philosophy*, need not be farther insisted on here.

Dr. Wollaston has divided the slider of a scale into the logarithmic spaces from 10 to 320, by a method familiar to all who are acquainted with the nature of logarithms, or in a similar manner as the line of numbers is laid down in Gunter's scale. He considers 10 as representing the weight of an atom of oxygen. On both sides of the scale he has written the substances most familiar to chemists, viz. the acids, bases, water, and principal salts, each opposite to the number on the slide, which corresponds with the weight of an atom of it. Suppose now we want to know how much oxygen combines with 200 mercury: bring 200 on the slide opposite to *mercury* on the scale; then over against *oxygen* on the slide will be found 16, the quantity of oxygen required. Suppose we want to know how much oxide of copper combines with 60 of sulphuric acid: bring sulphuric acid opposite to 60, and we shall find over against oxide of copper 61, the number required. The Sliding Rule is fitted to answer an infinite number of similar questions.

Dr. Wollaston determined experimentally the composition of nitrate of potash, and found it composed of 63 acid + 59 potash. His scale is referred to carbonate of lime, which he considers as the most exact standard of comparison that can be obtained.

On Thursday, the 11th of November, part of the Croonian lecture on the *Influence of the Nervous System on Muscular Motion*, by B. C. Brodie, Esq. was read. This lecture began with a short historical account of the facts and opinions of former physiologists on this subject. The author then relates a number of experiments which he himself instituted still farther to advance the subject. He found that after the destruction of the lower part of the spinal marrow of a dog, the arterial blood of a horse injected into the lower extremities of the animal, after circulating through the limb, came out at the mouths of the veins dark coloured. He found that the lower extremities of a frog, treated in the same way, though deprived of voluntary motion, contracted by stimuli and by the galvanic influence. He found that the heart of a rabbit continued to beat with its usual regularity for some time after the blood-vessels had been emptied of their contents; but that the action of the heart was destroyed when an animal was strangled, showing clearly that the stimulus of the blood is not the exciting cause of the action of the heart.

On Thursday, the 18th of November, the remainder of Mr. Brodie's paper on the *Influence of the Nervous System on Muscular Motion, particularly of the Heart*, was read. The author concluded with some general inferences from the experiments detailed in the preceding part of his paper.

Some farther experiments, comparing the Gregorian and Cassegrenian telescopes, by Brigade Major Cater, were also read. The author related a new experiment which he had made, and which confirmed the experiments detailed in his former paper on the superiority of the Cassegrenian above the Gregorian telescope.

At the same meeting there was also read an analysis of a new species of copper ore, from India, by Dr. Thomson. This ore was discovered about 13 years ago by Dr. Heyné, at some distance north-west from Madras. It occurs in nests in primitive trap. It is an anhydrous carbonate of copper, and was composed of the following constituents :—

Carbonic acid	16·70
Black oxide of copper	60·75
Red oxide of iron	19·50
Silica	2·10
Loss	0·95
	100·00

The silica was accidental, proceeding from small quartz crystals interspersed in the ore. The red oxide of iron was probably only mechanically mixed, as it differed in quantity in different specimens. Two other native species of carbonate of copper were already known ; namely, malachite and blue copper ore : but both of these are hydrates ; the first containing two integrant particles of water, the second one. The ore described in this paper was anhydrous.

LINNÆAN SOCIETY.

The Society resumed its meetings on the 2d of November. A specimen of convolvulus jalappa (*jalap* of the shops) raised in a stove in England was exhibited to the Society by A. B. Lambert, Esq.

Dr. Leach laid before the Society a shell (a species of Venus) found in the transition clay slate of Plymouth, and a similar shell found upon the top of Snowdon in a rock, presumed likewise to be transition slate. I may take this opportunity of mentioning, that I have been informed by the same Gentleman that he has seen madreporites in the transition limestone of Plymouth.

Mr. Sowerby presented to the Society a new genus of shell (a bivalve), found among the rubbish of the West India Docks.

A paper, by Benjamin Smith Barton, M.D. of Philadelphia, was read, describing a new species of Syren, found in Georgia, called the *Syren striata.* The paper contained likewise a description of another species, called Syren tetrapus.

There was read also a description of some plants growing on Mount Caucasus, by Mr. Stephen. This paper was sent from Moscow ; for the Natural History Society of that city, in consequence of its late destruction by fire, will not for some time be in a condition to resume the printing of their papers.

Part of a paper, by Dr. Leach, was likewise read, giving a corrected character of the genus *Meloe,* and describing six exotic species of that genus.

On the 16th of November the remainder of Dr. Leach's paper on different species of meloe was read. As this paper consisted of a technical description of different species of meloe, it is obviously not susceptible of abridgement.

Part of a paper, by the Rev. Patrick Keith, on the upward direction of the plumula, and downward direction of the radicle, of seeds, was also read. The paper began by stating the curious fact that, whatever be the position of the seed, the plumula always assumes an upward direction, and the radicle goes downwards. Then follows an historical statement of the different explanations of this curious fact given by philosophers. The supposed levity of the sap of the plumula, and gravity of that of the radicle, is mentioned; then the supposed attraction of air for the plumula, and of earth for the radicle. Dr. Darwin's opinion is next stated, and a direct experiment in refutation of it described.

GEOLOGICAL SOCIETY.

The Geological Society held its first meeting after the long vacation on Friday, the 5th of November. Mr. Webster's paper on the formations in the South of England lying over the chalk was continued. He described 18 different beds of calcareous matter, clay, and sand, most of which may be traced for a very considerable extent of country.

On the 19th of November Mr. Webster's paper was continued. He made a comparison between the different formations in the neighbourhood of Paris and those in the South of England, and showed their exact similarity, both in the constituents, position, and fossil remains which they contain; though some of the Parisian beds are wanting, or at least have not hitherto been observed in the South of England. Thus something similar to the lower part of the *calcaire grossiere* of France has been observed in England, but nothing similar to the upper part of that formation. The gypsum in Paris occurs in a fresh-water formation. Selenite is found in a similar position in England: but we have no rocks exactly similar to the gypsum of Montmartre. One of the most remarkable formations which lie over the chalk in England is the London clay, which varies from 200 to 500 feet in thickness. It extends over a considerable part of the north coast of Kent, and constitutes the surface of most of Essex and Suffolk. The fossil remains which it contains are various, and highly interesting. Mr. Webster gave a catalogue of the fresh-water shells found in the fresh-water formation in England, and showed that they are precisely the same that are found in the same formation in the neighbourhood of Paris.

At the same meeting a short notice, by Mr. Arthur Aikin, was read, respecting the granite of the Malvern Hills, one of which he ascended. He found it composed of quartz, felspar, mica, and pistazite; and would have hesitated to call it granite, had it not been for the numerous specimens of small grained graphic granite, which he found every where around.

Article XII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

I. *Pyrodmalite.*

Some years ago a mineral was discovered in the mine of Bjelke in Vermeland, a province of Sweden, situated on the north side of the lake Venner. This mineral was observed to have the property of giving out the odour of muriatic acid when heated. On that account it was distinguished by the name of *pyrodmalite.* The following description of it, which was drawn up by Assessor I. G. Gahn, of Fahlun, is translated from Hisinger's *Samling till en Mineralogisk Geografi öfver Sverige,* p. 175.

The colour of *pyrodmalite* is commonly yellowish brown, passing into greenish; internally, it is light greenish yellow. It occurs crystallized in regular six-sided prisms without any terminating pyramids. It is composed of plates lying upon each other in a direction perpendicular to the axis of the prism. Principal fracture, splendent; cross fracture, uneven, and without lustre: opaque: semihard: it is scratched by steel. The crystals are often several inches long. Specific gravity 3·081. Before the blow-pipe it becomes dark reddish brown, and emits the odour of muriatic acid. It then melts into a black slag, and at last a small bead is obtained, more or less attracted by the magnet. It dissolves readily, and in considerable quantity in glass of borax, and gives a colour indicating the presence of manganese and iron. In phosphate of ammonia-and-soda it dissolves with great difficulty. Its constituents are silica, lime, iron, manganese, and muriatic acid. It occurs in the mine of Bjelke mixed with iron ore, calcareous spar, and black crystallized malacolite.

Pyrodmalite was analysed last summer by Mr. Hisinger. He found its constituents as follows:—

Silica	35·4
Oxide of iron	32·6
Oxide of manganese	23·1
Alumina	0·6
Muriatic acid	6·5
Loss	1·8
	100·0

It would be difficult to form a conception of the composition of this mineral if it be a chemical compound. The muriatic acid can neither be united with the iron nor the manganese, as neither of these are in the requisite proportions. The escape of the acid by heat seems to indicate the presence of a portion of water amounting to about two-thirds of the loss stated as sustained in the analysis.

II. *Polychroite.*

This is the name given by Bouillon Lagrange, and Vogel, to the colouring matter of safron, formerly considered by chemists as extractive, but which they conceive to be a peculiar vegetable principle. To obtain it, we have only to digest saffron in water, evaporate the liquid to the consistence of a thick syrup, and digest this residue in alcohol. When the alcohol is evaporated, *polychroite* remains behind in a state of purity. The properties of this substance are as follows:—

1. It has a very intense yellow colour. Its taste is bitter, similar to that of saffron, and it has an agreeable smell.

2. It dissolves readily in water and alcohol, but scarcely in ether, and not at all in fat and volatile oils.

3. When the solution of *polychroite* is exposed to the light, it gradually loses its colour. Its colour is destroyed likewise by oxymuriatic acid. The addition of a few drops of sulphuric acid changes its colour to an intense and beautiful blue. Nitric acid, added in like manner, renders it green.

4. It combines with lime, potash, and barytes, forming with these bases soluble and insoluble compounds.

5. When sulphate of iron is dropped into a solution containing it, a dark brown precipitate is formed.

6. It stains cloth of an intense yellow colour.

7. When distilled it yields an acid liquid, a yellow coloured oil, and carbonic acid gas, and carbureted hydrogen gas. The acidulous liquid contains ammonia. The charcoal, when incinerated, leaves traces of carbonate, sulphate and muriate of potash, of carbonates of lime and magnesia, and of iron. See *Ann. de Chim.* vol. lxxx. p. 198.

III. *Picrotoxine.*

This is the name given by M. Boullay to a peculiar substance which he extracted from the *Cocculus Indicus*, to which that body owes its deleterious qualities. *Picrotoxine* may be obtained by the following process. Boil the seeds deprived of their pericarp in a sufficient quantity of water. Filter the decoction, and precipitate it by acetate of lead. Then filter again, and evaporate slowly to the consistence of an extract. Dissolve this extract in alcohol, and evaporate the solution to dryness. Repeat these solutions in alcohol and evaporations, till the residue is wholly soluble in alcohol and water. It then consists of picrotoxine mixed with a little colouring matter. Agitate it with a very small quantity of water, the colouring matter is dissolved, and the picrotoxine separates in small crystals. Its properties are as follows:—

1. Its colour is white, and it crystallizes in four-sided prisms.

2. Its taste is disgustingly bitter.

3. A hundred parts of boiling water dissolve four parts of picrotoxine, one half of which separates as the solution cools. The solution does not alter the colour of vegetable blues.

4. Alcohol of the specific gravity 0·810 dissolves the third of its weight of this substance. A little water throws down the picrotoxine; the addition of a greater quantity redissolves the precipitate.

5. Sulphuric ether of the specific gravity 0·700 dissolves only 0·4 of picrotoxine.

6. It is insoluble in oils, both fixed and volatile.

7. Diluted sulphuric acid does not act upon it; concentrated acid dissolves it, assuming a yellow colour. When heat is applied, the picrotoxine is charred and destroyed.

8. Nitric acid dissolves it without the disengagement of nitrous gas. The solution is yellowish green. When heat is applied, the picrotoxine is converted into oxalic acid; but about 18 parts of nitric acid are requisite to produce this effect.

9. Muriatic, oxymuriatic, and sulphurous acids, have no action on it.

10. Acetic acid dissolves it readily. Carbonate of potash precipitates it from this solution unaltered.

11. Potash, soda, and ammonia, diluted with ten times their weight of water, readily dissolve picrotoxine.

12. When triturated with potash, it assumes a yellow colour, but does not emit the odour of ammonia.

13. When heated, it burns without melting, or giving out flame, exhaling a white smoke, which has a resinous odour.

14. When distilled, it yields very little water and gaseous products, but much yellow coloured empyreumatic oil, and a brilliant bulky charcoal remains behind. See *Ann. de Chim.* vol. lxxx. p. 209.

IV. *Boletic Acid.*

This is a new vegetable acid obtained by Braconnot from the juice of the *boletus pseudo-igniarius* by the following process. The juice was boiled, filtered, and evaporated cautiously to the consistence of a syrup. This syrup was repeatedly digested in alcohol, the insoluble portion was dissolved in water, and precipitated by nitrate of lead. The white precipitate thus obtained was mixed with water, and decomposed by sulphureted hydrogen gas. The water being now evaporated yields numerous crystals, which constitute *boletic acid.* The properties of this acid are as follows:—

1. When purified by solution in alcohol and crystallization it is white, not altered by exposure to the air, and consists in irregular four-sided prisms.

2. Its taste is similar to that of tartar; it requires 180 times its weight of water to dissolve it at the temperature of 68°. It is soluble in 45 times its weight of alcohol

3. The aqueous solution reddens vegetable blues. Nitrate of lead occasions a precipitate in it which is redissolved by agitation. It precipitates the red oxide of iron completely from its solutions in the form of rust coloured flocks; but it does not throw down the black oxide of this metal. It precipitates nitrate of silver in the state of a white powder, which is soluble in nitric acid. Nitrate of mercury is precipitated in the same state; but the solution dissolves with difficulty in nitric acid. Neither lime nor barytes water produce any effect upon the aqueous solution of this acid.

4. When heated it rises in white vapours, which irritate the throat, and condense on surrounding bodies in the form of a farinaceous powder. When distilled the greatest part of it sublimes unaltered, excepting that it afterwards crystallizes more regularly. At the same time a little liquid appears, having a strong smell of acetic acid.

5. Boletate of ammonia is a salt which crystallizes in flat four-sided prisms, and is soluble in 26 times its weight of water at the temperature of 68°. Its taste is cooling, saline, and somewhat sharp. When heated it melts, swells, and sublimes. It precipitates red oxide of iron; but does not alter sulphates of lime, alumina, or manganese. It slowly precipitates nitrate of copper in blue silky needles.

6. Boletate of potash is very soluble in water, and crystallizes with difficulty. Acids precipitate the boletic acid from it.

7. When boletic acid is heated with carbonate of lime it dissolves it with effervescence. The boletate of lime crystallizes in flat four-sided prisms. This salt has little taste, and requires at least 110 times its weight of water, at the temperature of 72·5°, to dissolve it. It is decomposed by oxalic and sulphuric acids.

8. Boletate of barytes is an acidulous salt in white plates, little soluble in water or nitric acid. When thrown on a red-hot iron it burns rapidly, with a red flame, and striking scintillations, leaving for residue carbonate of barytes.

9. When heated with iron filings and water, hydrogen gas is emitted, and a yellow liquor is obtained with an inky taste. See *Ann. de Chim.* vol. lxxx. p. 272.

V. *Freezing of Alcohol.*

A correspondent informs me that the process followed by Mr. Hutton to freeze alcohol, and which he thought proper to conceal, was as follows. The alcohol is put into a condensing vessel, and air condensed on it as far as can be done with safety. The vessel is then reduced to as low a temperature as possible by means of freezing mixtures, and the air being allowed suddenly to make its escape increases the cold so much that the desired effect is produced.

VI. *Situation of Cryolite in Greenland.*

I received from Mr. Allan the following correction of his account of Greenland from Mr. Giesecké too late for insertion in the last Number of the *Annals of Philosophy*:—

"Cryolite occurs in two small detached beds, resting on gneiss; one of them is composed entirely of the fine white cryolite uncontaminated with any mixture; in the other, the brown variety occurs mixed with galena, pyrites, &c. They are situated very near each other. The first is touched at high water by the tide. It varies from 1 to 2½ feet in thickness. From the decompositions which this curious mineral has undergone, it could not be procured attached to the rock on which it rests. It is, besides, divided from it by a thin layer of mica, always in a state of disintegration. Mr. Giesecké is inclined to consider the cryolite as belonging to a floetz formation."

VII. *Meteorological Apparatus.*

In answer to the correspondent who wishes for correct information respecting Mr. Luke Howard's meteorological apparatus, I have only to refer to the wrapper of Number II. of the *Annals of Philosophy,* where every thing respecting it with which I am acquainted has been noticed. As to evaporation, he will find useful observations on it in Saussure, and in a paper by Mr. Dalton published in the Manchester Memoirs. The part of the apparatus most frequently wrong is the rain-gage, which never can give correct information unless it be placed within a few feet of the ground, and detached from all buildings. I believe the mean temperature indicated by the thermometer in most journals is too high. It must always be so, unless the lowest point to which the thermometer falls in the night be marked. This, in summer, is usually about sun-rise; in winter, it is irregular, depending on the wind. A good Six's thermometer I conceive to be a necessary appendage to every meteorological apparatus.

VIII. *Biddery Ware.*

I have been favoured by Dr. Wilkins with the following receipt, which he informs me is followed in making Biddery ware in some parts of India:—

Copper	16 oz.
Lead	4
Tin	2

Melt all these together; take 3 ounces of the alloy, and melt them with 16 ounces of zinc. This mixture constitutes the alloy of which Biddery ware is made. To give it a black colour, wash the surface with a solution of 1 oz. of sal ammoniac, ¼ oz. of saltpetre, ¼ oz. of common salt, ⅕ oz. of blue vitriol, and dip the Biddery ware into this solution.

IX. *Gong.*

Gongs are manufactured in Canton in an open manner. The largest kinds are made in one of the interior provinces of China. They are not made in any part of India.

ARTICLE XIII.

List of Patents.

John Brazill, of Great Yarmouth; for a machine for working capstans and pumps on board ships, which machine may also be applied to various other useful purposes. Dated September 4, 1813.

John Westwood, of Sheffield; for a new method of embossing ivory by pressure. Dated September 9, 1813.

Frank Parkinson, of Kingston-upon-Hull, distiller; for a still and boiler for preventing accidents by fire, and for preserving spirits and other articles from waste in the operation of distilling and boiling. Dated September 4, 1813.

Henry Liston, Minister of Ecclesmachen, Linlithgow; for certain improvements on the plough. Dated September 23, 1813.

Henry Osburn, Whitmore-house, Warwickshire; for a method of making tools for tapering of cylinders of different descriptions, made of iron, steel, metal, or mixture of metals; and also for tapering bars of iron, steel, metal, or mixture of metals. Dated October 15, 1813.

Robertson Buchanan, Glasgow, civil engineer; for certain improvements in the means of propelling vessels, boats, barges, and rafts, which may also be applied to the moving of water wheels and wind mills, the raising of water, the dredging, cleansing, or deepening of rivers and harbours, and the impelling of other machinery. Dated October 18, 1813.

Article XIV.

METEOROLOGICAL JOURNAL.

		Barometer.			Thermometer.					
1813.	Wind.	Max.	Min.	Med.	Max.	Min.	Med.	Evap.	Rain.	
10th Mo.										
Oct. 16	W	29·20	28·74	28·970	51	38	44·5	—	·19	☾
17	S W	29·01	28·64	28·825	52	42	47·0	—	8	
18	N W	29·55	29·44	29·495	58	35	46·5	—		
19	Var.	29·66	29·55	29·605	50	27	38·5	—		
20	E	29·66	29·50	29·580	52	42	47·0	·12	·14	
21	Var.	29·76	29·50	29·630	54	43	48·5	—	—	
22	N E	29·89	29·45	29·670	53	40	46·5	—	·10	
23	E	29·91	29·86	29·885	52	41	51·5	·12		
24	N E	29·86	29·82	29·840	57	45	51·0	—		●
25	N E	30·12	29·82	29·970	53	37	45·0	—	—	
26	N E	30·12	29·85	29·985	47	36	41·5	—		
27	N E	29·79	29·77	29·780	41	34	37·5	·17	6	
28	N	29·90	29·79	29·845	46	33	39·5	—		
29	E	29·90	29·70	29·800	47	27	37·0	—	—	
20	Var.	29·70	29·00	29·350	52	30	41·0	—	·50	
31	W.	29·25	29·00	29·125	56	30	43·0	—	—	
11th Mo.										
Nov. 1	W	29·68	29·25	29·465	49	31	40·0	—	—	☾
2	N W	29·83	29·41	29·620	49	35	42·0	8	9	
3	S W	30·32	29·83	30·075	51	29	40·0	—	—	
4	W	30·34	30·32	30·330	45	27	36·0	—		
5	S	30·32	29·95	30·135	47	29	38·0	—		
6	E	29·95	29·70	29·825	45	35	40·0	·14	6	
7	S W	29·70	29·37	29·535	53	41	47·0	—	·17	
8	S W	29·42	29·37	29·395	58	43	50·5	—	·22	○
9	W	29·62	29·42	29·520	51	42	46·5	—	—	
10	W	29·60	29·48	29·540	57	43	50·0	—	·10	
11	S W	29·65	29·51	29·580	54	44	49·0	·17	—	
12	W	29·49	29·45	29·470	54	32	43·0	—	·35	
13	S W	29·58	29·49	29·535	46	29	37·5	—		
14	W	29·58	29·17	29·375	43	33	38·0	3	8	
		30.34	28·74	29·625	58	27	43·41	0·83	2·14	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

REMARKS.

Tenth Month.—16. Fine morning: wet p. m.: lightning in the evening. 17. Showery. 18. Fine a. m.: shower p. m. 19. Hoar frost: fair day, after misty morning. 20. Cloudy: a. m. much wind at E. 22. About half-past seven p. m. a bright blue meteor appeared in the N. and passing to the westward with a steady and rather slow motion became extinct. There were some traces of *Cirrostratus* clouds, which increased afterwards. 23. Maximum of temp. at nine a. m.: cloudy, with a breeze. 24. Overcast. 25. Cloudy, a. m.: clear, p. m. During the twilight there was an abundance of red haze, first in the E. horizon, over clouds in that quarter; then at a considerable elevation in the W. It ended more clear, and orange coloured. 26. Cloudy at intervals. 27. a. m. Windy. 28. Hoar frost; which was found, on examination, to consist, not of spiculæ attached to the herbage, but of the drops of dew, frozen clear and solid. 29. Hoar frost: *Cumulostrati*, followed by *Nimbi*, one of the latter approaching from the E.: at four p. m. exhibited a double rainbow, on a ground of purple. 30. Spicular hoar frost: very misty: clear at noon: *Cirrostratus*, and a little rain, p. m.: wet and stormy night. 31. Windy, a. m.: *Cirrus*, with *Cumulus*: a shower about four, with a fine bow.

Eleventh Month.—1. Hoar frost. 2. Granular hoar frost: very clear sun-rise: clouds at noon: rain, p. m.: very windy night. 3. Sunshine, a. m.: wind, N.N.W.: a shower, p. m. 6. Calm clear weather, with hoar frost, these three days: the wind now rising. 7. Small rain at intervals: a solar halo, p. m. of large diameter. 8. Fair, a. m. Before four p. m. dark *Nimbi*, and rain beginning: being on the south side of London, I was surprised with a flash of lightning and a sharp peal of thunder: about half-past six had a glimpse of a meteor passing to the W. It was said to lighten after this time. 8, 9. Windy. 11. Stormy night. 12. Windy and cloudy: p. m. wet. 13. Hoar frost. 14. The same, with crystallized rime on the shrubs.

RESULTS.

Winds Variable: the Easterly prevailed in the former, the Westerly in the latter part.

Barometer: greatest height ....30·34 inches;
Least ....28·74 inches;
Mean of the period........29·625 inches.

Thermometer: greatest height ....58°
Least ....27°
Mean of the period....43·41°

Evaporation, 0·83 inches. Rain, 2·14 inches.

Tottenham, Eleventh Month, 17, 1813. L. HOWARD.

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END OF VOL. II.

CORRIGENDA.

P. 47, line 9, *for* " Alumina 3·500" *read* " 2·136."

P. 48, line 6, *for* " Nickel 3·623 " *read* " 7·305."

P. 51, line 16, *for* " Deutoxide of Nickel 5·623 " *read* " 9·305."

P. 51, line 17, *for* " Peroxide of Nickel 6·623 " *read* " 10·305."

P. 338, Exper. I. and II. *dele* " Example of oscillation above-mentioned."

P. 338, Exper. IV. line 4, col. 3. *for* " 3·00 " *read* " 4·00;" and after " Example of oscillation above-mentioned," *add* " p. 336, note."

P. 340, line 4 from bottom, and p. 341, line 7 from top, *for* " in a given time," *read* " at a given time."

P. 343, line 23, *for* " one per cent." *read* " one-tenth per cent."

P. 344, line 9, *for* " three or four pints," *read* " three or four parts."

C. Baldwin, Printer,
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